

## Smoke But No Fire So Far

LORD ROTHSCHILD'S prescription for the future organization of civil scientific research in Britain seems to have excited so far only mild and even polite interest. The newspapers have reported without much comment the suggestion which is the nub of the Rothschild report (see *Nature*, 234, 169; 1971) that applied research carried out by the research councils should in future be paid for out of the budgets of the government departments most likely to benefit from the results, but there has been no sign so far of how the proposal will be allowed to influence the future pattern of expenditure. On balance, the research councils are likely to dig in their heels and resist the application of the principle, but some of their determination may have been undermined by one of the few public statements so far, that in which Sir Harold Himsworth, director of the Medical Research Council until two years ago, told readers of *The Times* that he, for one, was opposed to Rothschild. After all, there comes a point when excessively ardent defenders of an entrenched position make it impossible for others to adopt more moderate points of view. And, this is the flaw, Sir Harold Himsworth has chosen to resist Rothschild on the grounds that the Haldane doctrine, first spelled out half a century ago, is still appropriate to the management of British public expenditure on scientific research. The truth is, as some of Rothschild's sternest critics will agree, that the Haldane principle has become an archaic survival from the past.

What is the issue? Sir Harold says that the effect of what Lord Rothschild has proposed would be to transfer to government departments the initiative for developing new lines of research in such fields—where the Medical Research Council is concerned—as clinical and para-clinical medicine. He goes on to say that “departmental policy can only be based on knowledge that exists at the time it is framed” and that “at any time research may show that this is faulty and that, accordingly, official policy is mistaken”. In short, Sir Harold says that government departments have a vested interest to steer clear of research for fear of being shown up and that they are especially likely to avoid “those parts of their work that are most relevant to current issues”. It is not necessary to believe in the infallibility of government to know that this argument is and should become invalid. After all, the days are gone when research was entirely a novelty in government work. Although it may have been sensible in the early decades of the century to fear that government departments would so entirely misunderstand the function and potential value of scientific research that they would discount it altogether, time has shown even the most iconoclastic departments that there can be practical value in the work that scientists do. Not merely in Britain but elsewhere, government departments are anxiously waiting for such diverse benefits as improved methods of air traffic control, better understanding of the ways in which impoverished families become poor and the ways in which military technology may be transformed by scientific developments. And although scientific research is bound

in the long run to be subversive in the sense that it will introduce people (civil servants included) to notions which have previously not crossed their minds, there is very little evidence that the research councils in the past fifty years have been successful at confronting governments with awkward scientific appraisals of their immediate actions. On the contrary, for all Sir Harold Himsworth's protestations, the research councils have been exceedingly uncontentious in their dealings with government and in their handling of issues of public importance which might be illuminated by scientific research (and which are usually ignored). In short, the full-blooded defence of the Haldane principle that there should be a separation between publicly supported research and the government departments most likely to be affected is false.

Is it nevertheless possible that research councils separate from government departments may be more imaginative spurs to innovation? This is one of Sir Harold Himsworth's points. So far as performance is concerned, the record is patchy. The Medical Research Council itself, Sir Harold's old bailiwick, has been exceedingly bold in its determined support of molecular biology, but much less interested in more humdrum matters such as the efficient use of hospitals for the treatment of patients—the operational research of applied medicine, so to speak. The Agricultural Research Council has supported some splendid basic research in universities and in its own research units, but has made its reputation on practical developments of immediate value to British agriculture. So far, the Natural Environment Research Council has not been able to make a significant mark on the conduct of public affairs, partly through youth and partly because it is an exceedingly amorphous and even artificial entity. In the past few years, the Science Research Council has had a profound and valuable influence on the development of policy in higher education, while the Social Science Research Council is doing splendid work in the improvement of the standard of social science in Britain, but the research councils immediately threatened, if that is the word, by the Rothschild proposals could probably have been every bit as imaginative as they have been if it had been necessary for them to win support for individual projects from sponsoring departments.

So why not adopt the Rothschild recipe in its entirety? The difficulty, of course, is not a matter of principle but of practice. For every objection to the Rothschild recipe, there is a countervailing argument in its favour. And who will deny that it would be extremely valuable for everybody if British government departments were required by circumstances to develop close relationships with scientific research laboratories? As things stand, however, nobody can be sure that the particular machinery described in the Rothschild report, which requires that government departments should develop ways of formulating their needs of scientific research, will function as efficiently as they might. This is why the first response should be not a decision of principle between the conflicting proposals of Sir Frederick Dainton and Lord Rothschild but a care-



fully calculated experiment to demonstrate the effectiveness or otherwise of the proposed links between the departments and the research councils. The best place to start is with the Agricultural Research Council. An obvious field of research is plant breeding, where both the Ministry of Agriculture, Fisheries and Food and the Agricultural Research Council are at present spending money. The object should be not so much to save on expenditure—only ten per cent of the Agricultural Research Council's budget would be affected—but to see whether a change-over from the present system to one in which the interaction between the research council and the government departments could be organized efficiently.

But who would ever be in a position to tell? One of the gaps in Lord Rothschild's argument is that his framework for the future pattern of publicly supported research in Britain makes no provision for the involvement of members of the public except as taxpayers. It may be argued in his defence that the research councils (with the Social Science Research Council and, to a lesser extent,

the Science Research Council notable exceptions) have been culpably over-secretive about the planning of their work. Too often they have declined to justify in public even such important aspects of public policy as the setting up of research units separate from (if linked with) universities. Too often they have declined to give serious thought to defining their strategy. One of the dangers of the Rothschild recipe is that it will make it easier for the public servants directly concerned to conceal the kind of arguments by means of which amicable relations are to be established between customers and contractors. If anything, this is a time when there should be a move in the opposite direction, towards less secrecy and confidentiality. What this implies is a need somehow to devise ways of allowing outsiders to know more about the relations between the research councils and the government departments with which they are primarily concerned. Those who wish to see the research councils retain some semblance of independence under the new regime would be well advised to worry about this point.

## Keeping the Computer Flag Flying

NATIONAL pride, once satisfied by the creation of a national air line or a television network, now tends to centre on pieces of advanced technology. This is the spirit in which the House of Commons Select Committee on Science and Technology seems to have approached the question of how to stimulate the British computer industry, so that it is something of a relief that its report on the subject, published two weeks ago (see *Nature*, **234**, 117; 1971), contains good sense as well as flag-waving. The committee is certainly on sure ground when it says that the most effective way of using government funds to promote the health of one sector of industry is to spend money on research and development, not on the purchase of products at uncompetitive prices. There is less to be said for the committee's proposals for the administration of government policy in present circumstances. And it is painfully apparent that the committee has not been able to trim its arguments to suit the prospect of British entry into the European Communities. Where does flag-waving stop and international cooperation begin?

The Select Committee's blind sense of patriotism is most clearly expressed by the way in which it overlooks the question whether Britain might not be better off without an industry for manufacturing computer hardware. Instead, the committee arrives almost without argument at the conclusion that "a strong independent industry for the supply of equipment and services should be fostered within the United Kingdom". (In this connexion, "independent" implies independence from the several United States companies already established in Britain.) Unfortunately, this proposition is not nearly as self-evident to outsiders as it seems to have been to members of the committee. For a country such as Britain, computer technology can bring two kinds of benefits. First, there are the economies in business and government operation which the efficient use of computing machinery can bring. Second, there are the still less tangible benefits to be won from the existence of commercially successful and entirely

indigenous industry. In an ideal world, the Select Committee would have set about the difficult but important task of striking a balance between these partially opposing considerations. It is a great misfortune that the occasion has been allowed to slip.

What might have been said? In spite of the wave of disillusion which has recently afflicted potential purchasers of computers, British investment in computing equipment—software as well as hardware—has in the past few years been running at about five per cent of industrial capital investment. That is a measure both of the importance of computing machinery and of the need that commercial and public organizations should be able to buy what suits them at prices which are as low as possible. Plainly it is an important part of the government's responsibility to foster use of the new technology, wherever the equipment may have been manufactured. The important question is to know how much energy to devote to that good cause and how much to divert to the building up of a computer industry with roots in national soil.

The case for supporting a strictly British computer industry is not self-evident, to say the best of it. For several years, the chief instrument of the government's policy has been the company now known as International Computers Limited, with a turnover of £130 million in 1970, forty per cent of it abroad. Although the government has in the past three years provided £13 million of research and development money (in return for five per cent of the equity), the company is smaller than the British subsidiary of IBM, which had sales of £170 million in 1970. In spite of the preference which ICL has been given in bidding for government contracts, which has for practical purposes shut out the subsidiaries of American companies, imports of computing machinery have grown quickly. ICL's performance overseas has been creditable, but there is enough internal evidence about the British market for computing machinery to suggest that the products of other manufacturers have



important competitive advantages. It is true that this balance may easily be changed by the performance of the new range of time-sharing computers on which ICL has been working, but that remains a hope and not a promise. In the meantime, it must be a serious question whether ICL will be able to deliver all the goods which its potential customers will be looking for in the years ahead.

The Select Committee might also have asked itself what would happen if ICL were to vanish from the scene. The obvious danger that skill in the manufacturing of computers would be lost to Britain is unreal, for most of the 20,000 people now working in the computer industry are employed by other companies. In any case, skill resides increasingly in the software houses. The fear that the present adverse balance of payments on computers might deteriorate still further would also be countered, in part at least, by the willingness of the companies with headquarters overseas to fight for a larger share of what would be a richer honey-pot if government preference for the single British manufacturer were abolished. In this calculation, the difference between the amount of profit accruing to British-owned companies and to the British subsidiaries of foreign companies is of course entirely unimportant. What does matter is the possibility that a strictly British enterprise might provide a better foundation for exploiting by-products of computer manufacture or might be a more efficient trojan horse for carrying British manufacturers within the walls of the European Communities. The Select Committee left both these arguments strictly to one side.

What are the long-term benefits of a British computing industry? And how should the British Government exploit them? Quite sensibly, the Select Committee lays down the principle that government subvention should consist of support for research and development, not protection against the harsh winds of competition, but it promptly goes back on itself with the proposal that there should be a Computer Purchasing Board responsible for coordinating government procurement and with instructions to give preference to "suppliers where the controlling interest is held by UK nationals". In the first place, it is doubtful whether a single purchasing office is even now a sensible objective. In this respect the committee seems almost as ready as the government to think of computers as nine-day wonders. Purchasing is at present coordinated by a strange amalgam of committees, with Her Majesty's Stationery Office looking on. Is there any substitute, in what are supposed to be days of professional management in government, for an arrangement under which the government's agency of financial administration — the Civil Service Department — arbitrates between users? Is it sensible to recommend a system of national preference without providing some guidance about the degree of preference which is justifiable? And if this unwelcome doctrine is to be stomachied at all, might it not be wise to think of it in a European context? In the long run, a sensible pattern of high technology in Europe will emerge only when European governments are not ashamed to buy each other's products, which argues for European and not national protection.

The thinnest part of the Select Committee's report is that dealing with research and development. To be sure, the committee is entirely right to complain that the British Government's spending on research and development,

most probably less than £6 million a year, is far too small. On the other hand, it is brash to suggest that government expenditure on research and development should promptly be raised to £50 million a year, roughly ten times the present scale of expenditure. One obvious difficulty is that there is very little experience to suggest how such expenditure should be organized. The time has gone when military contracts could help with the development of civil computers. In Britain, the attempt in the past few years to use the National Physical Laboratory as a focus for creative research and development has not been a roaring success. There might be something to be said for an identifiable planning body, representing government and industry, to work out a specific policy for research in computing and related fields, but the suggestion that there should be a Computer Research and Development Board consisting of five independent people with a licence to spend £50 million a year is a mixture of wishful thinking and irresponsibility.

If there are funds on such a scale available, it would almost certainly be better to spend them on setting up an organization to provide the software without which computer purchasers cannot use the machines they buy. To the extent that software is an essential in selling hardware, such a development would also suit the manufacturers. The immediate need, in other words, is for a suitably strong commercial organization. In the long run, the best hope may be to persuade government departments to put out to tender more of their software development. That may be the best of all the Select Committee's proposals.

## 100 Years Ago



### The Origin of Insects

IN an article by Dr. Beale, in your number for Nov. 23, on "One of the Greatest Difficulties of Darwinism," a most extraordinary misconception is stated to be a difficulty. That the pupa state is a modification of the ordinary process of skin-shedding in the Insecta is proved by so many facts, that one cannot understand for a moment how it can possibly be denied, much less how its denial can be made use of as an argument against the doctrine of evolution. Sir John Lubbock pointed out long ago that, in the development of the Insecta, every grade of modification exists between those insects which are gradually developed, each successive ecdysis producing only the slightest possible modifications, and those which undergo a change so complete that it may be likened to the process of metagenesis, as it has been called, which takes place in the Echinodermata.

It is an utter mistake to suppose that any insect is redeveloped during the pupa state. The most perfect instance of metamorphosis is that of the flies (some Diptera). In these the materials out of which the perfect insect is developed are supplied by the breaking up of the muscular system and fat bodies of the larva; but the cellular structures known as the Imaginal discs of Weismann are formed in the egg, and persist all through the life of the larva. These, it is true, only form a skin or case in which the fly is developed; but they are really nothing more than a larva skin, formed on the inside of the larva skin in the egg, and detached from it by the subsequent modifications of the larva.

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# What Future for High Energy Electron Physics ?

ONE of the proposals for discussion at today's (December 3) meeting of the European Committee on Future Accelerators (ECFA) in Geneva is that an electron accelerator should be built to cater for the needs of European physicists in the late 1970s and 1980s. The most likely outcome is that a working party will be set up to examine the feasibility of the project.

The proposal will be put by Professor A. Ashmore, director of Daresbury Nuclear Physics Laboratory, who knows as well as anybody that the Science Research Council, having agreed to support the 300 GeV accelerator now being built at Cern, will not also be able to finance the 20 GeV synchrotron that had been planned for Daresbury and for which the present 5.0 GeV electron accelerator (NINA) would have been an injector. Professor Ashmore is also only being realistic in now pressing for a European rather than a British machine, as the cost of independence in such a large project is becoming increasingly prohibitive for any country.

Professor Ashmore's proposal is likely to have a mixed reception although the affiliated ECFA countries are certain to agree, on scientific grounds alone, that there is an obvious need for a high energy electron accelerator. But feet will drag because the proposal comes so soon after the allocation of money for the 300 GeV accelerator. The possibility of alienating public opinion is the fear among the diplomats of high energy physics. But a feasibility study would entail little financial drain on any national budget, and its terms of reference are unlikely to be more specific than the consideration of the type of accelerator needed in Europe ten years from now.

The future of the Daresbury laboratory and the Rutherford High Energy Laboratory at Chilton seems to be assured for the next five years, although it is not clear what will happen afterwards. In 1972-73 the Rutherford laboratory will cost the SRC £7.2 million and the Daresbury laboratory £3.2 million—essentially the same as they will cost in 1976-77. Budgets are reviewed annually, however, and there is always a chance that support might be withdrawn before then. In the current year, the SRC will contribute £1.2 million towards the con-

struction of the 300 GeV machine and £7.0 million for experiments with the present facilities at Cern. Five years from now, £5.8 million is earmarked for the 300 GeV machine and £3.2 million for the other facilities. Inflation apart, the total British contribution to CERN will be kept constant at £9.0 million a year after 1972-73.

Manpower threatens to be as restricting as money. Unless there is an increase in the number of high energy physicists within a year or so, there will be too few British scientists to make effective use of both international facilities and Cern accelerators. There are at present 350 high energy physicists in Britain of whom 80 are electron physicists at Daresbury. About half of the total are graduate students and according to Professor J. C. Gunn, University of Glasgow, chairman of the nuclear physics board of the SRC, there are no plans to increase this number.

It is not possible at present to say whether either the Rutherford or the Daresbury laboratories—or even both—will have to be closed to allow full British participation in the 300 GeV project, which will need 1,000 scientists when it is in full operation. A decision will have to be made before 1976, however, and electron physicists will be more directly affected than the present users of NIMROD or the 28 GeV proton synchrotron at Cern. This is why the people directly concerned are anxious that the course of electron high energy physics for the next decade

be marked out as soon as possible. The only machine in Europe comparable with NINA is the 6 GeV electron synchrotron at the Deutsches Elektronen-Synchrotron laboratory in Hamburg (DESY), but that laboratory is at present being improved by the addition of two storage rings that will raise the maximum energy available to 9 GeV.

High energy physicists are pressing for a decision on the future of electron physics against their better judgment. They realize that unless a plan for the future is agreed on soon, there is a danger of an unbridgeable gap between the end of NINA's useful little life—or much sooner if it is decided that NINA be sacrificed on the altar of the 300 GeV machine—and the time when a new facility could become operational. Even if a decision were taken now to go ahead with a design study on a European electron accelerator, there would be a hiatus after useful physics on NINA is brought to an end. Professor Ashmore is concerned that the hiatus does not develop into an unbridgeable chasm.

What are the alternatives facing the electron physicists if NINA, for one reason or other, is axed? One promising suggestion involves an electron beam obtained from a proton accelerator. In this way, for example, 100 GeV electrons can be obtained from the reaction

$$\pi^0 \rightarrow 2\gamma \rightarrow e^+ + e^-$$

if the  $\pi^0$  particles are produced by 300 GeV protons. These electrons can be



Fig. 1 Daresbury Nuclear Physics Laboratory.



used to study the deep inelastic continuum with an energy that is not at present a practical proposition with a conventional electron accelerator. The drawbacks are that the beam intensity would be low and that its optical quality would leave much to be desired, but the high energy is an attraction.

It is also possible that British electron physicists could collaborate after the end of NINA's useful life with either the scientists at DESY or at the Stanford Linear Accelerator Center. Such associations, however, could only be a temporary arrangement to bridge a gap before an accelerator that British physicists could use by right became operational.

A suggestion at present circulating among high energy physicists does not involve the building of a new facility. It is proposed that a small electron accelerator—perhaps of a few GeV—should be used to inject electrons into the intersecting storage rings (ISR) at Cern. The advocates of this scheme already have a machine lined up—the 2.5 GeV accelerator nearing the end of its useful life at the University of Bonn. Such a combination of the ISR and a small electron machine could produce electrons of 15 GeV in the centre of mass system, but the intensity of the beam would be low, and large aperture detectors can compensate only to a certain extent. Even so, a detailed study of this proposal needs to be carried out.

Nothing less than a new accelerator that will produce a beam of several hundred microamps of electrons will satisfy most high energy electron physicists, even if in the long run they may have to make do with something less. To win allegiance in the 1980s, an electron accelerator will have to produce at least 40 GeV electrons. How might that be done? A report published last December by M. C. Crowley-Milling of the Daresbury laboratory (*High Energy Electron Accelerators: Cost Comparisons*, DNPL, TM/78) compares the cost of constructing different types of electron accelerators. The summary of his calculations, shown in Fig. 2, reveals several interesting facets, not the least of which is the competitive cost of superconducting linear accelerators. His calculations show that a synchrotron is the cheapest way to produce an electron energy up to 35 or 40 GeV. For 40 GeV and above, there is no clear choice and estimates for synchrotrons, superconducting linear accelerators and hybrid combinations of synchrotrons and linear accelerators become similar—all being about £30 million (compared with the £100 million for the 300 GeV facility at Cern).

The optimistic estimate for the superconducting linac (c) is based on a voltage gradient of 10 MV per foot, whereas the other estimate (b) is based on a gradient

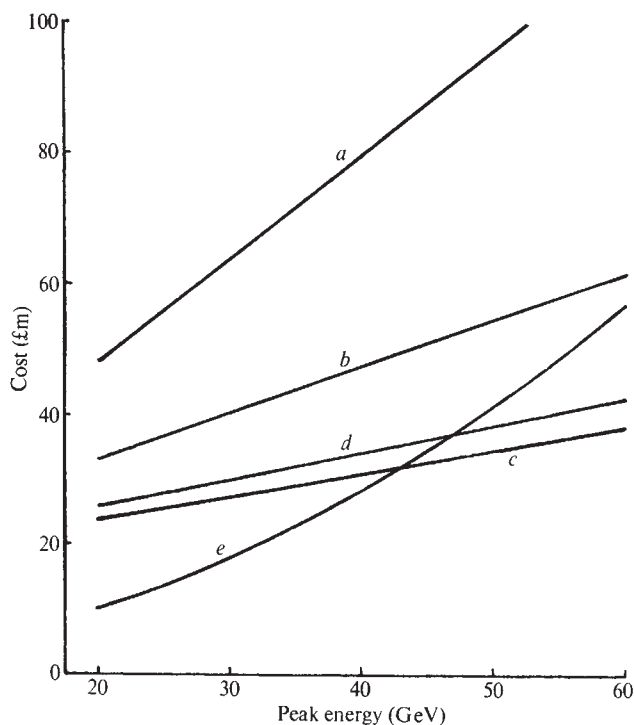


Fig. 2 Cost of electron accelerators after M. C. Crowley-Milling. *a*, Stanford Linear Accelerator; *b*, superconducting linear accelerator; *c*, superconducting linac (optimistic); *d*, mesotron; *e*, synchrotron.

of 4.3 MV per foot—the aim at the High Energy Physics Laboratory (HEPL) at Stanford University.

The accuracy of these estimates, made before any practical experience had been available from superconducting machines, is being questioned because of the setbacks now being experienced at HEPL. Professor H. Alan Schwettman confirmed last week that there are problems with the voltage gradient in the accelerator. The sections of the machine already installed have been run at gradients of 1 MV per foot—a long way short of the planned 4 MV per foot—and there have been difficulties in raising the voltage because of magnetically weak points on the cavities and excessive field emission.

There is cheerful news, however, in that 10–20 microamps have already been run through the test sections—not far short of the 100 microamps the accelerator is designed to carry. Superconducting accelerators are not out of the woods yet, however, and time—and possibly an infusion of money—will be needed before the Stanford machine achieves its design rating. At present it looks as if the SLAC estimates of 10 MV per foot are exceedingly optimistic and that superconducting linear accelerators will continue to be more expensive than conventional synchrotrons for a long time.

Perhaps the most interesting and practical accelerator to achieve electron energies greater than 40 GeV is a synchrotron-linear accelerator combination that has been called a mesotron. In

such a machine, two linear accelerators are connected by a series of bending magnets to return the beam through the machine several times. The advantage of a mesotron over the synchrotron is that it is much easier and cheaper to increase the energy at a later date.

Another possibility would be to build a synchrotron to give 30 GeV to 40 GeV, but to increase this energy at a later date by putting in radio frequency power. A more economic way of doing this would be to inject the extracted beam into a linear accelerator to give a final energy of 50 GeV at a much lower cost than needed to obtain this energy by a linear accelerator or a synchrotron alone.

The situation will be much clearer if and when a decision is needed on a European accelerator. In particular the success or failure of the Stanford HEPL accelerator project will have been assured and the cost will then be less of an estimate. At this stage it is certainly opportune to ask whether another accelerator is in the best interests of the high energy community in Britain. Without doubt, involvement in a further European accelerator project will ensure that British national laboratories will no longer house large accelerators. There will not be enough people to man them, to say the least of things. An electron accelerator at 40 GeV or more can be justified on scientific grounds alone, but the 300 GeV project has shown that scientific merit is not a deciding factor when governments are asked to finance such expensive projects.

## OLD WORLD

UNESCO

### Work for Pruning Hooks

THE discussion of Unesco's Man and the Biosphere programme in Paris last month resulted in the thirty-one research projects that the conference met to discuss being cut to thirteen. The thirty-one projects were the result of a Unesco general conference as long ago as November 1966 which authorized the preparation of a research programme on the rational use and conservation of the resources of the biosphere. The thirteen projects which have finally emerged will now be considered by the twenty-five national committees of the participating nations which will decide which projects they will support and to what extent.

Before the meeting, doubts had been expressed about the effective need of the programme, particularly in Britain and the USA, but the reaction of the British delegates to the conference—although this may not prove to be the reaction of the national committees—is that the meeting was effective and constructive. Dr M. E. D. Poore, Director of the Nature Conservancy, declared himself "quite well satisfied with progress made" as the programme that emerged is much more precisely defined and practicable. The thirteen projects that have emerged range from studies of ecosystems in rivers, islands, mountains and forests to studies on the effects of engineering work, energy use and population movements on man and his environment.

During the conference, the emphasis changed from considering man set against the biosphere to considering man as an integral part of it who simply has a greater power than other organisms to alter it for better or worse. The relationship between the natural sciences and the social sciences received greater emphasis in the final projects—one is concerned with how man's perception of environmental quality varies from place to place.

One reason the conference was so successful is that the delegates, although representing governments, were largely scientists rather than administrators, and thus the final programme is felt to be more practicable and profitable than might otherwise have been the case.

No cost estimates are available for the projects as yet, and it is doubtful if they will involve large amounts of expenditure that would not otherwise have occurred. Where the programme is likely to have greatest effect is in the exchange of findings between countries and in the prevention of duplication. Additional finance may well be provided by developed countries working in

participation with developing countries to initiate studies on, for example, desert and forest ecosystems. This was the basic philosophy on which the International Biological Programme—which is not government sponsored—ran, and of which the Man and the Biosphere programme is to a certain extent the continuation.

Once the national committees have decided the extent of their involvement a detailed programme will be produced for confirmation by another conference in March 1973, although work on the projects may well start before then.

INFLUENZA

### Beating the Bug

BRITISH health authorities expect outbreaks of influenza this winter to be worse than last year's comparatively light outbreaks. Large numbers of cases have already been reported on the continent, particularly in Spain and eastern Europe, and *Pravda* reported last week that the Soviet Union is expecting outbreaks soon. In Britain, people at special risk have been advised to arrange vaccination quickly. These include people with chronic lung complaints, heart conditions and glandular disorders. The influenza strain is the same as last year's—the A2 Hongkong virus which first appeared in 1968 and which affected eight million Britons two years ago. Vaccines at present available give protection against all current strains of influenza for about seven months. An attack of influenza, however, gives immunity to that particular strain for between six months and two years, which partly accounts for the light outbreak last year. Antibody levels in the population last year meant that about 70 per cent of the population had some sort of protection but this has now fallen to about 40 per cent.

There are three serotypes of influenza (A, B and C), each with subtypes. Antibody levels against type C are naturally high and at worst it only produces mild colds. The A and B strains are the ones that produce the epidemics. Both these strains mutate slowly and continuously so that new vaccines have to be introduced every few years to allow for the change, but A strains also mutate abruptly every seven to ten years—as in 1968 with A2 Hongkong and in 1957 with A2 Asian. It is these sudden mutations that tend to produce the really bad epidemics as it takes three to four months to produce commercial quantities of a new vaccine. From studies on pigs which have caught A2 Hongkong from man it is thought that the A strain influenzas may be animal viruses which have crossed the species barrier into man.

The vaccines used in Britain are the

### Satellite Delays

THE launching of the ESRO satellite, HEOS-A2, scheduled for December 10 has been postponed until January 1972 at the earliest. The reason is that NASA is still investigating the cause of the recent failure of the ITOS-B launch and no Delta rocket launchings will take place until the end of the investigation. NASA has confirmed that in spite of the delay it will still attempt to launch the next scheduled ESRO satellite at the end of February, as originally planned.

The launching of the UK-4 satellite originally planned for November 29 has been put back once again. The launch, which has now been twice postponed because of a fault in a monitor in the University of Iowa experiment, is now planned to take place from the Vandenberg test base in California on December 9.

inactive type. Formalin or betapropiolactone is used to inactivate the virus, leaving the antigens still present on the surface to build up the blood's antibodies. The vaccines do not, however, guarantee immunity. The Department of Health and Social Security says they are only 40 to 60 per cent effective but the manufacturers claim an efficiency of about 70 per cent. The discrepancy is probably due to the conditions under which the vaccine is tested and to the fact that a number of conditions produce symptoms similar to those of influenza and these cases are unjustly put down to vaccine failure.

Perhaps the best hope in the control of influenza lies in live virus vaccines which are already in use with mixed success in the Soviet Union. Work on live vaccines began in Britain in 1957 at the Common Cold Research Unit in Salisbury. The principal problem is that a virus in a vaccine must be sufficiently attenuated not to give the recipient influenza and yet active enough to be effective. Live vaccines under tests are producing immunity levels of almost 100 per cent, although they produce lower antibody levels than inactive vaccines. Commercial application of live vaccine is still some years away, however, as careful tests are needed to ensure that the attenuated virus does not revert to its wild state during use. If and when live vaccines become available they will be much cheaper than the inactive vaccines as about 10,000 doses can be produced from one egg as compared with one dose of inactive vaccine.



## NEW WORLD

### HIGHER EDUCATION

## Funding in Limbo

by our Washington Correspondent

CAUGHT in the usual pre-Christmas backlog of legislation, the Senate has decided to put off until January at the earliest further consideration of bills designed to increase funding for institutions and students in higher education. The chief reason for the delay is that early in November the House of Representatives passed a bill that is so different from legislation passed by the Senate in August that there will be a lengthy fight in the Senate even before a conference committee is given the formidable task of sorting out differences between the bills. Consequently, the whole issue has been sent back to the Senate Committee on Labor and Public Welfare for its consideration.

Apart from the sharply differing philosophies that are embodied in the versions of the bill passed by the House and the Senate, senators are concerned about the fact that measures for desegregating schools, and in particular amendments opposing the busing of children from one school to another to achieve a racial mix, were tacked onto the bill passed by the House. The Senate passed its own desegregation bill earlier this year, and will probably combine the two pieces of legislation to have a consistent policy to take to a conference committee. In addition, Senators who are opposed to busing have promised to try to put the anti-busing amendments passed by the House into the Senate bill.

While these skirmishes over desegregation are going on in Congress, college administrators will be anxiously looking on to see how Congress will deal with their interests. The chief point to be resolved is whether the conference committee will finally decide to distribute federal support for higher education in the form of grants to institutions or of grants to students. The House version called for general grants to institutions, distributed largely on the basis of the total number of students at each university or college, while the Senate bill would establish the right of each qualified student to receive \$1,400 minus the amount that his family would be expected to contribute.

Although colleges will not be badly affected by the delay in agreeing on the legislation, since appropriations for 1972 have already been agreed on, a long delay next year would present some difficulties for planning budgets for the 1972-73 academic session. Those involved in the legislation are predicting that it will take several weeks to sort out the differences.

## Horrendous Problems

IN his last annual report as President of Massachusetts Institute of Technology, Dr Howard W. Johnson has issued a stern warning of the financial problems that lie ahead for higher education. Dr Johnson's bitterness at past funding policies in higher education is evidenced by these extracts from his annual report for 1970-71:

"I have spoken in each of my annual reports on the problems of financing MIT. We are a special place, and we fare a bit better than many others. But the financial problems ahead for higher education are horrendous. Increasing tuition must continue in the face of rising costs, but I, for one, fear that such a continuation will bear most heavily on the large middle group of our students that fall in the financial range between the very needy and the very affluent. I fear that a steadily rising collegiate

budget could eventually prevent many of them from attending MIT. There is a very real need here that has not received adequate attention by anyone in higher education.

"We must maximize our efforts at raising private resources. The satisfaction of investment in private education is still there despite the occasional uneasiness for some that accompanies the support of youth. But quality institutions need federal support in addition, and new forms of that support must be devised if the long-term interests of our society are to be protected."

Dr Johnson's warnings could well be directed at the Administration which should, if a conference committee can sort out the differences in bills passed by the House and the Senate, soon be considering a Congressional proposal to give federal grants to institutions of higher education.

## Short Notes

### Enzyme Detergents

A COMMITTEE appointed by the National Research Council—the operating arm of the National Academy of Sciences—has come to the conclusion that "the average enzyme detergent laundry product in normal use by consumers has not produced more primary irritation of the skin than have similar products that contain no enzymes". Commissioned by the Food and Drug Administration, the study bases its conclusion on the fact that only six cases in which there is unequivocal evidence of allergic reactions associated with use of enzyme detergents came to the committee's attention during the course of its extensive investigations.

The committee found, however, that some workers in plants that manufacture detergent products who have been exposed to relatively high concentrations of dust containing enzyme materials have suffered respiratory tract and skin injuries. The symptoms range from irritation and coughing to breathlessness, sometimes lasting for several hours. But the committee suggests that the average consumer would be exposed to such low doses of the enzyme materials that he or she would not be affected. The study has found immediate acceptance in at least one quarter—the day after the study was published, Procter and Gamble issued a press release saying that "we hope these findings by the nation's most respected scientific body will serve as the ultimate reassurance to housewives and will finally put to rest the unfounded

charges which have been made against the consumer safety of enzymes". The report was one of the first of the NRC publications to bear the new label to the effect that responsibility for all aspects of the report rests with the study committee.

### Technology and Foreign Policy

THE National Science Foundation has awarded the Brookings Institution a grant of \$125,000 to study "the impact of technological change on American foreign policy and prospects for world order". The study, which will be directed by two members of the Brookings foreign policy staff, will be concerned with a broad range of questions such as new ways for exploiting seabed resources, telecommunications, and weather prediction and modification. Environmental pollution and the depletion of scarce resources are two other areas which will come under study. The chief object of the exercise will be to determine for each area studied what new forms of international organization and regulation will be required.

### New Man at OST

EDWARD E. DAVID jun, the President's Science Adviser, has a new special assistant for national security matters. The first incumbent of the newly created post will be John B. Walsh, a former member of the Secretary of the Air Force's research and development staff, and a specialist in missile systems. Walsh will work directly with David and with Dr Henry Kissinger, Assistant to the President for National Security Affairs.



# Where To Go After Mars

by our Washington Correspondent

FLUSHED with the success of Mariner-9's encounter with Mars, space scientists are again beginning to talk with some conviction about the merits of taking a closer look at Jupiter and the outermost planets of the solar system. Soon after Mariner began sending photographs of the Martian dust storm, for example, Dr Robert S. Kraemer, director of NASA's planetary programmes, took the opportunity to call a press conference to remind the public that Pioneer 10 is scheduled for launch to Jupiter early next year, to reach the planet in December 1974, and that it would be followed a year later by Pioneer 11.

These flights, Dr Kraemer reminded his audience, are essential to chart part of the course for the much more ambitious "Grand Tour" of the planets—Jupiter, Saturn, Uranus, Neptune and Pluto—to be launched between 1976 and 1979 (Jupiter, Saturn and Pluto first and then Jupiter, Uranus and Neptune). One much publicized feature of the Grand Tour is that the special alignment of the outer planets that enables one spacecraft to visit three planets in succession comes round once every 179 years.

The recent flurry of interest drummed up in the Grand Tour possibility is being widely interpreted as an indication that NASA officials are trying to sell the idea to the Office of Management and Budget in time to get an increase in funding for development of the TOPS (thermoelectric outer planet spacecraft) in the 1973 budget request. Negotiations between NASA and OMB are now in a critical stage, and if the 1976 and 1977 launches are to go ahead, there will have to be a strong financial commitment to the programme in the near future. NASA officials should be helped in their negotiations by a study published last week by the National Academy of Sciences\* which concludes that exploration of the outer solar system should be "one of the major objectives of space science in this decade".

But there is no complete agreement within the scientific community—or even among planetary scientists themselves—on the merits of pressing ahead with the Grand Tour. The broad argument is whether the Grand Tour would soak up so much of NASA's scientific budget that the whole programme would get out of

balance, with smaller but worthy projects being squeezed out. Some planetary scientists also argue that a programme based on an intensive study of Jupiter would pay a better scientific dividend than would a quick look at the outer planets.

In a great many respects, the academy report takes account of all these points of view, for while tub thumping for the Grand Tour, it also points out that the

project should not be undertaken at the expense of intensive study of Jupiter nor at the expense of other scientific projects in NASA's overall programme. But perhaps the chief value of the report is that it presents NASA with a range of options from the full-scale Grand Tour to a watered down version, depending on the level of available funds. A report on priorities in space research, published by the academy

## Hard Way to Jupiter

by our Washington Correspondent

IN February or March next year, Pioneer 10 will be launched on a journey that should take it past the orbit of Mars, through the asteroid belt, to Jupiter and then on to the edges of the solar system. The chief object of the exercise is to take a close look at Jupiter, but just how close the look should be has developed into something of a controversy. Present plans are to take the spacecraft in to about three Jupiter radii, but the working group on outer planetary research of the Space Science Board has suggested that it should be brought as close to the surface as quarantine specifications allow—about 1.3 Jupiter radii. Even at that distance the intensity of the particle flux trapped in the planet's magnetic field may be so great that the craft's instruments could be put out of action.

Why should planetary scientists suggest that a spacecraft costing nearly \$100 million should be destroyed by taking it so close to Jupiter? The argument hinges on the importance of assessing the intensity of Jupiter's radiation, to see what sort of a hazard it poses to later exploration of the planet or even to the Grand Tour. Those who argue for taking Pioneer 10 close to the surface point out that even if the instruments are destroyed, most of the scientific work would already have been done, and the choice is really between studying Jupiter's radiation belts or the interplanetary medium beyond Jupiter. Since Pioneer is not designed for a long lifetime, it is difficult to predict how much of the interplanetary medium would be surveyed before its signals become too weak to pick up.

The question was, however, brought up during a meeting of planetary scientists at NASA's Ames Research Center on November 11. According to Dr Charles F. Hall, project manager for the Pioneer programme, the consensus of that meeting was that the distance of closest approach should be kept at about three Jupiter radii on the basis that although the absolute level of the energy and flux of protons in the radiation belts are poorly known, the rate of change of energy with distance from the planet is much better known. In other words, the information relayed back from Pioneer 10 can be extrapolated to determine absolute radiation levels closer to the planet.

Radiation damage is, however, only one of the hazards the Pioneer 10 and other craft venturing much beyond the orbit of Mars must encounter. Before the spacecraft arrives at Jupiter, it must spend about six months crossing the asteroid belt, where it stands about a ten per cent chance of severe or even fatal damage. During its passage through this belt, two instruments will be set the task of scanning asteroids and measuring the mass and energy of tiny particles impinging on the spacecraft.

Once through the asteroid belt, Pioneer 10 will travel on to Jupiter, reaching the planet in December 1974. Instruments will sample the proton and electron flux, and measure the energy output from the planet with an infrared radiometer, which should also map the temperature distribution and provide some answers about Jupiter's enigmatic red spot, and a magnetometer will measure the planet's magnetic field. Beyond Jupiter, the spacecraft will send back data on the interplanetary medium and cosmic rays, until its signals become too weak to monitor when it reaches about 2,000 million miles past Jupiter.

\*Outer Planets Exploration 1972-1985, Space Science Board, 2101 Constitution Avenue, Washington DC 20418.

earlier this year, said in effect that the Grand Tour should not be undertaken unless NASA's budget for scientific research were to increase by 50 per cent—a criterion that placed the project lower on the shopping list than most planetary scientists would like.

Prepared by a study group of the Space Science Board (a committee of the National Academy of Sciences that provides advice and commentary to NASA on space science), under the chairmanship of Dr Francis S. Johnson, of the University of Texas, the report addresses itself specifically to the question of whether or not the chief thrust of exploration of the outer planets should be to undertake an intensive study of Jupiter or to flit round all the outer planets, taking a first look at each. The philosophical argument boils down to the practical question of how best to apply a limited amount of money. The Grand Tourers argue the value of a quick look at hitherto uncharted planets and their satellites, especially in preparation for fuller studies. The special alignment of the planets in the 1970s and 1980s also provides a unique opportunity to get to the outer planets quickly and relatively cheaply, and such a mission would also keep plenty of steam in the planetary exploration programme.

Those who argue for a more intensive study of Jupiter (and to a lesser extent Saturn) suggest that there are already theories about the planet that are crying out for direct observation, and that Jupiter has many features—such as a magnetic field and a magnetosphere—which merit intensive study. And there is also the prospect for taking a close look at the satellites of Jupiter and Saturn.

Linked with the recommendation for more intensive study of Jupiter and Saturn as opposed to the Grand Tour is the suggestion that, even though the opportunity for taking in several planets and their satellites in one mission will not recur before the middle of the 22nd century, nuclear electric powered spacecraft, which would considerably shorten flight times to the outer planets, could be developed in the 1980s. The chance of studying the outer planets will therefore not be completely foreclosed by not taking up the Grand Tour option.

The latest academy report attempts to reconcile these points of view by sailing a middle course, concluding that "a programme combining TOPS Grand Tour with the more intensive study of Jupiter and Saturn with flybys, probes, and orbiters is fully justified". Specifically, the study group recommends that if NASA's budget for planetary exploration were to increase to about \$400 million a year, four TOPS Grand Tour missions should be flown between 1976 and 1980, two of which would carry entry probes. Alongside this pro-

gramme, however, the study group suggests that Pioneer-type spacecraft should be developed for carrying probes and orbiters to Jupiter and Saturn, with about two launches a year.

An important peg in the study group's recommendations is that the exploration of Jupiter and Saturn with the less sophisticated Pioneer-type spacecraft should not be subject to the budgetary axe to accommodate the Grand Tour, and accordingly the report recommends a series of cuts in the Grand Tour programme as the budgetary horizons for planetary research contract. Essentially what the report suggests is that the number of missions should be reduced to three or to two, in preference to reducing the payload, but that as a last resort the payload should be cut down to 130 lb. If NASA were to embark on even this scaled down version of the Grand Tour, however, it would require a quite substantial increase in the funds devoted to planetary research, and the study group suggests that the floor of the outer planetary research programme should be a study of Jupiter with Pioneer-level spacecraft, including turbopause probes and perhaps deep entry probes to Jupiter and Saturn, and orbiters to Jupiter. At all budgetary levels, the report suggests that the seven-segment Titan rocket should be developed, and that nuclear electric and solar electric propulsion should receive some funding to provide the capability to reach the outermost planets in the 1980s.

These recommendations are subtly different from a set of options listed in a major report of the Space Science Board published in March this year (see *Nature*, 230, 142; 1971). That report set out to provide NASA with a list of priorities in its space science budget, given different levels of funding, and it suggested that the Grand Tour should not be considered unless NASA's space science budget were to increase by 50 per cent—in effect, the report recommended the full Grand Tour or nothing, and given the present economic climate, Grand Tour enthusiasts knew they would get nothing. The latest report, however, provides a more flexible approach including a watered down but still scientifically exciting economy class Grand Tour—the scaled down version could be accommodated without unbalancing the space science programme if NASA's space science budget were to grow to 125 per cent of its present value, the study group suggests.

Key feature of the Grand Tour project is the TOPS spacecraft—a thermo-electric spacecraft that must have a lifetime of at least ten years, a computer that can test and repair itself, and a capability for carrying scientific payloads of at least 130 lb. Full development costs are expected to be about \$440

million, and conversion to probe carrier and orbiter will add about another \$20 million and \$130 million respectively to the bill. Because the Grand Tour option is tied to a specific date, the most crucial question now emerging is how long a full-scale start on the TOPS spacecraft can be delayed before the option on the Grand Tour is missed.

NASA's budget for the present financial year contains \$20 million for development of TOPS, but the project has not yet had Congressional approval. If the 1976–77 launch window is not to be missed, there will clearly have to be a larger financial commitment to TOPS development in the budget now being put together in the White House, with the understanding that the budget for 1974 would include another sharp increase. Whether the OMB, let alone Congress, is prepared to swallow such an increase is doubtful at present, but to judge by the recent proselytizing of NASA officials, the agency is clearly staking a considerable amount on outer planetary exploration.

What effect is the academy's study likely to have on the prospects for the Grand Tour? Even the chief participants in the study are unwilling to answer that question, but it seems likely that one effect will be that NASA will not let the Grand Tour go ahead to the exclusion of other projects—both the priorities report and the latest report were insistent on that point. Dr Herbert Friedman, who was chairman of the executive committee which produced the priorities report, said last week, for example, that "Dr Fletcher is very objective about all of these questions and insists that his people pay high regard to external advice from sources such as the National Academy", and Dr Michael B. McElroy of Harvard University said that he would be "very disappointed if we did not take advantage of what is essentially a unique opportunity"—but quickly added that he would be even more disappointed if in going for the Grand Tour option the space science programme became unbalanced.

Similar sentiments were also expressed by Dr George Wetherill, of the University of California at Los Angeles. Pointing out that to miss the Grand Tour option would take a greater deal of steam out of planetary exploration, and that the option of developing a nuclear electric spacecraft for outer planetary exploration would probably delay the exploration of planets beyond Saturn until the 1990s, Dr Wetherill said that the Grand Tour offers a unique opportunity for space research. "One area that has not been given adequate discussion," Dr Wetherill suggested, "is the question of priorities in the whole NASA programme, for example, between the Grand Tour and the shuttle."



## NEWS AND VIEWS

# Neuronal Uptake Processes

It is now well established that the transmission of information in the nervous system generally involves chemical rather than electrical relays. At synaptic junctions the transmission of impulses is effected by the release of minute amounts of chemical transmitter substances which diffuse across the narrow gap separating the presynaptic and postsynaptic cells to trigger either excitation or inhibition of activity in the postsynaptic element. Acetylcholine and noradrenaline have long been known as the transmitters involved in the peripheral nervous system, but in the central nervous system it seems likely that these substances play only minor parts in the chemical transmission of impulses at synapses. The amino-acids such as gamma-aminobutyric acid (GABA), glycine, glutamate and aspartate probably represent the more important transmitter substances in the brain and spinal cord.

The use of an amino-acid, such as glycine or glutamate, as a neurotransmitter poses certain problems because in addition to this postulated specialized function these substances are also present in all living cells and serve important functions in metabolism, and as precursors for protein synthesis. The neurones that use such amino-acids as transmitters must therefore be faced with certain delicate logistical problems in the handling of the amino-acids for several different functions within the same cell.

The ubiquitous occurrence of amino-acids such as glycine or glutamate in all the cellular elements of the central nervous system also poses very difficult technical problems to the biochemist who is interested in studying the small compartment of amino-acid likely to be associated with neurotransmitter functions. On page 297 of this issue of *Nature*, however, Logan and Snyder suggest one way in which such studies may be made possible. These workers have studied *in vitro* the uptake of radioactive amino-acids by nerve terminal particles ("synaptosomes") in homogenates of rat brain and spinal cord. Analysis of the kinetics of amino-acid transport for seven common amino-acids revealed that in spinal cord homogenates three amino-acids—glycine, glutamic acid and aspartic acid—showed two kinetically distinct components in their uptake. One component was of relatively low affinity (apparent  $K_m$  approximately 1–4 mM) whereas a second component could be distinguished as having a much higher affinity ( $K_m$  approximately 0.02 mM).

In the spinal cord there is evidence that all three amino-acids may serve a transmitter role. The other four amino-acids which Logan and Snyder tested—alanine, arginine, leucine and serine—showed only a single component in their uptake in both spinal cord and cerebral cortex homogenates. This component was of the "low affinity" type. In the cerebral cortex homogenates, glycine exhibited only the low affinity component in its uptake; other evidence suggests that this amino-acid functions as a transmitter only in the spinal cord and lower brain stem. There is thus an apparent correlation between a functional role of amino-acids as neurotransmitters and the existence of high affinity uptake processes. Similar high affinity uptake processes have been described for

other neurotransmitters, such as noradrenaline, serotonin, dopamine and GABA (for review see Iversen, *Brit. J. Pharmac.*, **41**, 571; 1971). The special high affinity uptake process for glycine in spinal cord was also described in slices of this tissue by Neal and Pickles (*Nature*, **222**, 679; 1969) and Neal (*J. Physiol.*, **215**, 103; 1971).

In the case of the uptake of noradrenaline, the uptake process is known to be associated with adrenergic nerve terminals and seems to have an important physiological function in terminating the released transmitter by a process of "recapture". There is also evidence that the uptakes of dopamine and serotonin are specifically associated with neurones which use these substances as neurotransmitters. Recent autoradiographic studies on the location of radioactive GABA after its uptake by brain slices and homogenates have indicated that this uptake may also be associated with a specific population of neurones, possibly corresponding to those which use this amino-acid transmitter (Hökfelt, *Brain Res.*, **22**, 391; 1970; and Bloom and Iversen, *Nature*, **229**, 628; 1971).

There is thus some reason for optimism in hoping that the special uptake processes associated with the transmitter functions of amino-acids in the central nervous system may prove invaluable studies of these systems. It is possible that uptake processes represent the mechanisms by which transmitter actions at synapses are generally terminated, in which case the metabolic inactivation of acetylcholine at cholinergic synapses will prove to be the exception rather than the rule. Whatever the physiological significance of such processes is, there seems little doubt that they will prove to be extremely valuable for investigations of the transmitter functions of amino-acids in the central nervous system.

The existence of high affinity uptake processes for common amino-acids such as glycine and glutamate suggests that it may be possible to achieve a selective incorporation of the labelled forms of these amino-acids into the transmitter pools, provided appropriate concentrations of exogenous amino-acid are used. At saturating concentrations uptake will occur far more rapidly by the high affinity components than by the low affinity systems. Logan and Snyder's article which reveals the existence of high affinity uptake systems for glutamate, aspartate and glycine in nerve terminals is thus potentially of considerable significance to workers interested in neurochemical studies of transmitter systems in the central nervous system.

## Phase Transitions go Bang

THE notion that earthquakes and some other geological processes, volcanoes for example, may be triggered off by the explosive release of energy is a little like an explanation in search of something to explain. On page 293 of this issue of *Nature*, Professor A. F. M. Barton and his colleagues return to a theme which seems first to have

been started by Professor A. R. Ubbelohde more than twenty years ago. Although the specific energy release in the solid-solid phase transitions likely to occur in the Earth's crust is small, when cubic kilometres of material may be involved, it is clear that very large amounts of energy may be made available. Moreover, in several laboratory systems—Barton *et al.* give a list of some of these, the polymorphic transitions in antimony and of austenite to martensite, for example—it does seem that phase transitions can be rapid, sometimes explosively so.

Most contributors to this discussion have assumed that in the crust or mantle of the Earth, a phase transition would spread through a body of rock consisting of a metastable phase as would a shock wave. But where would the metastable phases have come from? The suggestion now is that the way in which the material of the oceanic crust is carried beneath continental margins in the process of continental drifting provides a continually replenished source of metastable rocks, the energy released from which may provide the driving force for some of the earthquakes which occur with foci at considerable depths beneath the continental margin. The explanation is ingenious and attractive. The greatest difficulty is that of reconciling the pattern of energy release recorded by seismometers, which usually implies that shearing mechanisms are responsible for the deep focus earthquakes, with the notion that energy might be released in large volumes of material by explosive phase transitions. In other words, there seems to be a source of energy in the mantle but no geophysical evidence to suggest why it should be invoked.

Is it necessary to let such an intriguing possibility go begging? For some time now, it has been clear that more needs to be done to understand the consequences of phase transition in geological processes. Earlier this year, Dr D. R. Waldbaum (*Nature*, **232**, 545; 1971) argued that temperature changes accompanying the phase transitions in the mantle might be of great importance. His particular point was that the Joule-Thomson coefficients for many of the materials of which the mantle is made are likely to be negative at the pressures which occur in the Earth's crust, which im-

plies that adiabatic decompression would bring about an increase of temperature and the release of heat even when no phase transitions are involved.

Although there is some dispute about the precise significance of Waldbaum's argument and about the likelihood that adiabatic processes do actually occur in the mantle of the Earth, this is yet another means by which the appearance of large amounts of energy deep in the interior of the Earth may be explained. This

is why it seems quite clear that there is a need for more detailed and more accurate studies of the thermodynamic properties of rocks at high pressures as well as of the kinetics of phase transition in bulk materials.

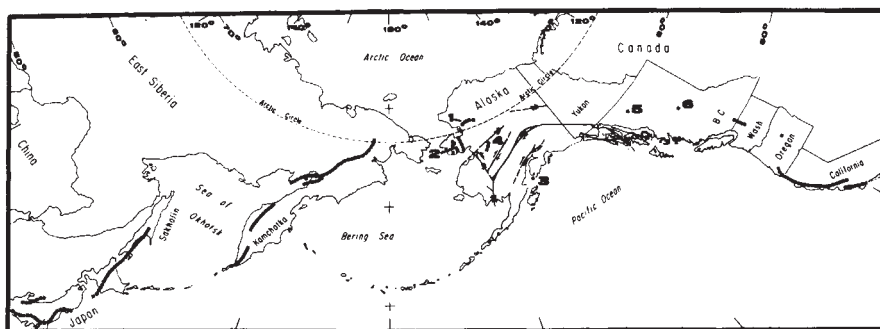
For the time being, these are the directions in which speculation about explosive phase transformations in geology might be given a firmer basis. In short, even if there is not yet a crying need for the use of these explanations, they may at least serve as stimulants of valuable research.

## Tectonic Implications of Blueschists

A FEW years ago Blake *et al.* (*Tectonophysics*, **8**, 242; 1969) noted that blueschist facies rocks seem to occur in narrow tectonic belts in association with ultramafic rocks; and this fact led them to suggest that the metamorphism which produced them may have occurred during underthrusting in a zone of anomalously high water pressure. If so, such metamorphism may be expected to occur most frequently along continental margins where oceanic crust is underthrusting and being subsumed, or even where such processes once took place but have now ceased. Such an expectation is especially attractive because blueschist facies metamorphic belts are also known to be associated often with ophiolites—an association thought to be characteristic of oceanic crust.

metamorphic belts current recognized, including the Alaska terranes (1–4) and those recently discovered in Canada (5–6) by Monger (*Geol. Surv. Canada Paper* 68–48, p. 12; 1969) and Paterson (unpublished hitherto).

This is not the first time that blueschists have been discovered in Alaska; but what Forbes and his colleagues have done is to show that the scale of the Alaskan blueschist terranes is far greater than previously suspected. In fact, there now seems to be one large belt of blueschist metamorphism stretching from the Seward Peninsula (site 2) to the Baird Mountains (site 1). The Kaiyuh Mountain site (4), on the other hand, seems to form an isolated terrane although later discovery may link it to the Seldovia terrane at site 3.



Map (polar projection) of western North America, north-east Siberia, Kamchatka and Japan (Hokkaido and Honshu), showing the regional distribution and location of blueschist facies metamorphic belts.

In next Monday's *Nature Physical Science* these speculations are placed into some sort of perspective by Forbes *et al.*, who report new discoveries of blueschists in Alaska and use these discoveries to show that blueschist terranes form a well-defined belt around the northern Pacific margin. The accompanying map shows the blueschist facies

Forbes and his colleagues say that their report is a preliminary statement of the new discoveries; and thus they do not discuss tectonic implications in any detail. What does seem to be clear, however, from the distribution of blueschist metamorphism around the Pacific is that it is the result of global, rather than purely local, tectonic processes.



## ENZYMES

## Push-button Control

from our Molecular Biology Correspondent

ENZYMOLOGY is clearly getting harder every day. Time was when one enzyme was considered to do one job, and the only legitimate objective of the enzymologist was to find out how. An increasing number of enzymes are now coming to light which can be switched on, off, or even into other modes, in response to triggering ligands. A very advanced model is that described by Meister and his colleagues (Trotta *et al.*, *Proc. US Nat. Acad. Sci.*, **68**, 2599; 1971). This is a carbamyl phosphate synthetase which, like other members of the family of glutamine amidotransferases, utilizes the amide group of glutamine for various synthetic functions. It is a characteristic of this group of enzymes that they are also able to make do with ammonia, though this is turned over with lower efficiency.

The *Escherichia coli* carbamyl phosphate synthetase puts together its end-product from glutamine, bicarbonate and ATP and intermediate steps in this process can also be isolated. The enzyme is controlled by several positive and negative feedback ligands, which are formed at the ends of the synthetic pathways in which it is involved. Trotta *et al.* have now demonstrated that the molecule consists of two subunits, estimated by detergent-gel electrophoresis to have molecular weights of 42,000 and 130,000. These chains could be dissociated, separated, and then recombined to give a substantially native product.

Examination of the activities of the separated subunits showed the following: neither chain alone was capable of the synthesis of carbamyl phosphate from glutamine, nor of the partial reaction of glutamylhydroxamate hydrolysis in the presence of ATP and bicarbonate. The large subunit, on the other hand, was active in carbamyl phosphate synthesis from ammonia rather than glutamine, and also in another partial reaction—the hydrolysis of ATP in the presence of bicarbonate. Both chains individually retained the glutaminase function. The various activators and inhibitors still exert their control over the activity of the large subunit, which therefore evidently contains the relevant binding sites. The function of the small subunit is surmised to be the binding of glutamine to a site at which it can be attacked by a nucleophile, which by an affinity label experiment with a glutamine analogue is inferred to be a thiol group. Trotta *et al.* suggest from a comparison of related enzymes in other species that the small subunit may represent an evolutionary refinement, in which

ammonia was supplanted as the main nitrogen source by glutamine. A test of this hypothesis would be a structural comparison of glutamine-binding chains from the related enzymes of other species.

A system with great biochemical appeal just now are the cyclic AMP-dependent protein kinases, which have been identified in a number of tissues. These seem always to consist of two subunits, one catalytic and one regulatory, and the word now is that the cyclic AMP operates by binding to the latter. In Krebs's laboratory a direct demonstration of this scheme in the rabbit skeletal muscle enzyme has now been achieved (Brostrom *et al.*, *ibid.*, 2444). Two kinases, differing somewhat in molecular weight, are present in the tissue. Each consists of a regulatory and a catalytic subunit, and it is only the former which differ in size. Mole for mole the two proteins are identical in AMP binding capacity. When cyclic AMP is added to the enzyme, the subunits dissociate, and can be separated chromatographically, the ligand being then found only on the regulatory chain. Short of denaturation, the bound cyclic AMP could not be removed from the protein, except by addition of an excess of the catalytic subunit, when it was forthwith displaced.

It seems then that there the system functions by a disproportionation mechanism: cyclic AMP will bind to the regulatory subunit, and thereby displace it from its partner, or the catalytic subunit may bind to the regulatory

chain, with displacement of the cyclic AMP. In practical terms the liganded regulatory subunit when added to the intact kinase increases the cyclic AMP concentration required for a given level of kinase activity. If some agency (insulin being an interesting candidate) were to cause a diminution in cyclic AMP concentration, the equilibrium between the kinase subunits would shift in favour of association, and the activity would become more sensitive to the cyclic AMP that remains. Brostrom *et al.* suggest that the complex with the regulatory subunit may be the form in which the cyclic AMP is kept in readiness in the cell. The next step will be to measure the concentrations of the two subunits actually present.

The protein kinases in liver have been studied by Chen and Walsh (*Biochemistry*, **10**, 3614; 1971). They are three in number, two, of apparently identical molecular weight, being cyclic-AMP-controlled, and a smaller one, which is not. The preponderant heavy component is heterogeneous, in that catalytic subunit can be resolved into two components. The smaller kinase is evidently an isolated catalytic chain, but is not identical with the other catalytic subunits. On the other hand, it will combine functionally with isolated regulatory subunits. Chen and Walsh note that there are many synthetic functions in the liver which might be expected to be regulated by cyclic AMP, and might therefore call for the presence of different specialized protein kinases.

## Peptide Map of Chimpanzee Haemoglobin

THE haemoglobins of higher primates are very like those of man. In the chimpanzee it is thought that all the three major polypeptide chains ( $\alpha$ ,  $\beta$  and  $\gamma$  chains, the last of which is the foetal analogue of the  $\beta$  chain) have identical sequences to the corresponding human molecules. Moreover, the  $\gamma$ -chain exists in two forms, one with glycine, the other with alanine at position 136, in monkeys, apes and man. Again the chimpanzee possesses the minor adult component,  $A_2$ , which has the structure  $\alpha_2\delta_2$ , just as in humans, and it is the  $\delta$ -chains which are the subject of a sequence study by De Jong, reported in next Wednesday's issue of *Nature New Biology*.

The tryptic peptide map of chimpanzee  $\delta$ -chains is identical with that of human  $\delta$ -chains, except for one peptide. The substitution turns out to be val-126 for the met-126 found in man. It is highly probable that there are no other differences between the sequences. The

replacement corresponds to a point mutation (GUG→AUG) which would have occurred after evolutionary divergence, valine occupying the corresponding position in the molecule in all known primate  $\alpha$ ,  $\beta$  and  $\gamma$ -chains, and also in the  $\delta$ -chains of some New World monkeys. (Old World monkeys seem to have no haemoglobin  $A_2$ .)

Calculations have shown that the rate of evolutionary mutation in mammalian haemoglobins is one substitution for 100 residues every  $10^7$  years. With 725 residues in the normal haemoglobin chains ( $\alpha$ ,  $\beta$ , two types of  $\gamma$  and the  $\delta$ ) altogether, the statistically predicted extent of mutation would be one residue between all of them every 1.4 million years. The divergence time for hominids is variously reckoned to be between 4 and 14 million years ago, and De Jong argues that the absence of any replacements whatever since divergence is good evidence in favour of the shorter time.

## PALAEOANTHROPOLOGY

**Descent of Man**

from a Correspondent

THE hundredth anniversary of the publication of Darwin's *The Descent of Man* set the theme for the symposium for the study of human biology held on November 19 at the Hospital for Sick Children, London. Professor J. S. Weiner (London School of Hygiene and Tropical Medicine), in a retrospective book review of Darwin's second major work, showed Darwin's remarkable foresight in predicting many features of human evolution which have subsequently been verified. Darwin's suggestion that man arose from a tropical African ape has found firm support not only in the fossil record but also in the more esoteric fields of molecular and biochemical investigations.

Dr D. Ferembach (University of Paris) was unable to attend, but his contribution, which suggested that in man and certain animals bone growth and bone morphology can be affected by dietary and caloric deficiencies during crucial growth periods, was read for him. Dr Ferembach questioned whether these effects may not have influenced morphological differences in living human groups. Dr C. Oxnard (University of Chicago), in a largely theoretical paper, discussed new analytical tools for the investigation of skeletal form. Medial axis transformation and Fourier optics were among the techniques he described as "optical dissection". Dr H. Preuschoft (University of Tübingen), in a biomechanical investigation of certain East African fossil primates, suggested that "*Proconsul*", an important Miocene form, was strictly quadrupedal with no locomotor specializations for arboreal life.

Dr G. Olivier (University of Paris), in a restatement of the concepts of nineteenth century French anthropology, discussed the relationship between "hominization" and brain development as reflected in cranial capacity. Dr J. Musgrave (University of Bristol), using the results of a multivariate analysis of the hand of Neanderthal man and certain Upper Palaeolithic *Homo sapiens*, suggested that the two groups showed important anatomical differences in hand structure (see *Nature*, **233**, 538; 1971). He further suggested that the Neanderthal hand, with its wide hand and short, broad phalanges, may have been cold adapted.

Dr M. H. Day (Middlesex Hospital Medical School) presented a discussion of Middle Pleistocene femora including the recently published *Homo erectus* femur from Olduvai Gorge (*Nature*, **232**, 383; 1971). The great similarity between Olduvai Hominid 28 and the Peking femora has thrown further doubt on the provenance and relationships of

the "*Pithecanthropus erectus* I" femur from Java. This specimen, which is stratigraphically associated with the *Homo erectus* skull cap from Trinil, has long been thought to show essentially modern morphology. Dr Day further suggested that the "*Pithecanthropus erectus* VI" femur, probably from Kedung Brubus, is not hominid at all and is possibly carnivore. Miss T. Molleson (British Museum (Natural History)) discussed a variety of analytical and relative dating techniques used on the Javan femora in an attempt to define more clearly their provenance and date. The tests, using fluorine, uranium and nitrogen levels, have all been inconclusive as far as dating is concerned.

Undoubtedly the most important occasion of the day was the presentation by Mme and Dr Henri de Lumley (Marseilles University) of the new Arago material. In an almost unprecedented gesture, the participants in the symposium were able to examine the original specimens. The Arago Cave is situated in the south-eastern French

Pyrenees near the town of Tautavel. In a Tayacian level, dated through its geology and fauna to the earliest Riss (about 200,000 BP), a partial skull with a complete face and two mandibles have been found. The parietals and occiput of the skull are missing and an apparent stone tool remains fixed by matrix within the skull cavity; the face shows only slight damage and distortion. This skull is of great importance because only two other faces of Middle Pleistocene times are known: one, Steinheim, is badly distorted and today, thirty-eight years after its discovery, it remains unprepared and inadequately described, and the second, "*Pithecanthropus* VIII" from Java, has only recently been reported and is so far incompletely prepared and described (S. Sartono, *Proc. Ned. Akad. Wet.*, Ser. B, No. 2; 1971). At present it is obviously impossible to discuss the affinities of the Arago material. Its attribution, by the de Lumleys, to a "pre-Neanderthal" group is attractive at this stage in that the term reflects what is quite possibly its transitional nature.

**Subsolidus Reduction of Lunar Spinel**

A FEATURE of lunar rocks that sets them apart from their terrestrial equivalents is that they crystallized under extremely low partial pressures of oxygen. This has resulted in the virtual absence of ferric iron and in the widespread occurrence of troilite (FeS) and metallic iron. A less obvious consequence now seems to be the reduction suffered by certain spinels after crystallization. Compositional variations in lunar spinels were recently discussed by S. E. Haggerty in *Nature Physical Science* (**233**, 156; 1971) and in a communication in next Monday's issue he enlarges on this aspect.

Apart from a small isolated group of chromian pleonasts, all lunar spinels so far described belong to a series extending, with decreasing titanium content, from ulvospinel ( $\text{Fe}_2\text{TiO}_4$ ) towards chromite ( $\text{FeCr}_2\text{O}_4$ ); in addition, there is some substitution of Mg for Fe and of Al for Cr. Re-examination of Apollo 12 material and a study of samples from Apollo 14 and 15 show the widespread occurrence of spinel-ilmenite-iron intergrowths. The ilmenite ( $\text{FeTiO}_3$ ) forms lamellae along octahedral planes in the spinel and is accompanied by blebs of free iron.

Haggerty interprets the intergrowths not as true ex-solution effects, which they resemble, but as subsolidus reduction. Not all spinels in the series exhibit these intergrowths but only those, in any one sample, towards the ulvospinel end—as revealed by probe analysis. Examples are found further towards the chromite end in samples

from Apollo 14 than in those from Apollo 15 or 12, and further still in fragments of crystalline rocks from breccias than in unfragmented rocks. The subsolidus reduction of the spinel meant that it became increasingly enriched in the chromite end-member as Fe and Ti were removed to form more and more metallic iron and ilmenite.

Consideration of available thermodynamic data allows the oxygen fugacity to be plotted against temperature for the dissociation of the pure end-members ulvospinel, ilmenite, hercynite ( $\text{FeAl}_2\text{O}_4$ ) and chromite, and shows that of these minerals chromite is the most stable, ulvospinel the least. Ulvospinel can dissociate either partially to ilmenite and iron, or completely to rutile and iron. The fact that only partial dissociation is observed would seem to set close limits on the fugacity/temperature conditions. In one sample examined, the decomposition of a fayalitic olivine ( $\text{Fe}_2\text{SiO}_4$ ) to symplectic metallic iron and cristobalite provides confirmation of a very low oxygen fugacity, as the theoretical dissociation curve lies between those for the partial and complete dissociation of ulvospinel.

Haggerty holds that the decomposition observed indicates lower oxygen fugacities than those prevailing during crystallization. The reduction may have occurred during initial deuteric cooling, but the fact that the breccias are more intensely reduced than the crystalline rocks suggests that it may be related to contact with meteoritic iron vaporized during impact events.



## CONTRACTILE PROTEINS

**Muscular Platelets**

from our Cell Biology Correspondent

Now that an actin-like protein, which may well prove to be completely identical to muscle actin, has been found in considerable amounts in nerve cells (*Nature New Biology*, **234**, 115; 1971) where it may be responsible for the motility of the growing tips of axons, it comes as no surprise to read in the current issue of the *Proceedings of the US National Academy of Sciences* (**68**, 2703; 1971) that Adelstein, Pollard, and Kuehl have isolated a myosin-like as well as an actin-like protein from human blood platelets.

Platelets play a crucial part in wound healing; they cause the retraction of clots and clearly such a contractile function must depend on some sort of molecular contractile apparatus. It is several years now since so-called thrombosthenin—a complex mixture of contractile proteins—was isolated from platelets but successive attempts to isolate actin or myosin-like proteins from this material have resulted in failure. Adelstein and his colleagues have, however, now succeeded in this by dissolving thrombosthenin in a buffer containing  $Mg^{2+}$ , ATP and dithiothreitol and fractionating it by ammonium sulphate precipitation followed by chromatography and polyacrylamide gel electrophoresis.

By these procedures they have isolated proteins which can be classified as myosin-like by several criteria. The myosin-like protein from platelets, for instance, binds both muscle and platelet actin; it has an  $Mg^{2+}$  dependent ATPase activity and in the electron microscope it can be seen to have the characteristic structure of myosin with a rod and a head portion, the head portion binding actin. Furthermore, the major polypeptide chain of platelet myosin has a molecular weight of about 200,000 daltons, the same as that of rabbit skeletal muscle myosin. As Adelstein *et al.* comment, the predominance of head and rod portions of myosin molecules in their preparations suggests that platelet myosin is probably subject to proteolytic cleavage either exclusively during its isolation or *in vivo* before isolation. The suggestion that thrombin may be responsible for the cleavage of thrombosthenin proteins in platelets has, of course, been made repeatedly in the past.

Apart from intact myosin and myosin head and rod pieces, the fourth chief protein which Adelstein *et al.* have isolated from thrombosthenin assembles into filaments 6 nm wide which form "arrowhead" complexes with platelet and skeletal myosin. This protein has the same molecular weight as skeletal muscle actin, 46,000 daltons,

and it contains one 3-methylhistidine residue per molecule. For these reasons Adelstein and his colleagues believe it is an actin.

Clearly, fingerprint analyses of these two platelet contractile proteins and, ultimately, analyses of their amino-acid sequences will have to be made in order to establish their complete identity or otherwise with their skeletal muscle counterparts. But from the evidence currently available it is reasonable to conclude that the molecular basis of contraction in platelets is in all essentials similar to that of skeletal muscle.

## MATERIAL SCIENCE

**Metal Matrix Composites**

from a Correspondent

THE objective of a symposium on November 15 at the Liverpool Polytechnic on the subject of metal matrix composite materials was to bring together research workers and potential consumers for an exchange of current views on the usefulness of these composite systems and possible future developments. It was clear that although experimental work (such as that described by Dr B. Harris of the University of Sussex) is still being carried out on metals reinforced with strong metal wires, present interest is largely in systems in which carbon fibres or refractory whiskers or fibres are the reinforcing agents. The obvious reason for this is the potential saving in weight

to be gained from using these light fibres instead of the denser metallic reinforcements.

The dominant theme of the symposium was undoubtedly the problem of the high temperature instability of carbon fibres and whiskers in contact with matrix metals such as nickel and aluminium. Experiments with carbon fibres have so far seemed to concentrate on the use of nickel or aluminium as the matrix metal. Dr S. J. Harris (University of Nottingham) explained that on the basis of cost, density and general properties, the aluminium/carbon fibre system is probably the most promising for study, and he described the methods used at Nottingham and Rolls Royce for producing good composites. He discussed the properties of these materials, in comparison with such competitors as boron fibre-reinforced aluminium and whisker-reinforced aluminium, but was forced to conclude that experimental strengths are often lower than theoretically obtainable values, usually because of fibre damage during manufacture.

Work on carbon fibres in nickel described by Dr W. Bonfield (Queen Mary College, London) and Mr P. Ormiston (University of Southampton) left listeners with the impression that the lack of high temperature compatibility in this system, which leads to fibre degradation especially in the presence of oxygen, probably precludes its commercial use unless some means could be found of introducing an efficient diffusion barrier between fibre and matrix. There was considerable discussion of the

**Specificity of mRNA Degradation**

How messenger RNAs are degraded in bacteria and how the degradation process is regulated remain vexed questions. There is, of course, no shortage of hypotheses and there are one or two enzymatic activities which are likely candidates for the job of cutting messenger RNAs to pieces, but the biochemistry of the process remains to be fully elucidated and its elucidation is clearly not going to be any easy matter. As Marrs and Yanofsky report in next Wednesday's *Nature New Biology*, different messengers in the same *Escherichia coli* cell are degraded at different rates and this observation leads them to suggest that the rate of degradation of any messenger may be partly determined by its particular structure.

Marrs and Yanofsky have measured the half-lives of phage T7 mRNAs and tryptophan operon messengers in *E. coli* cells infected with an amber mutant of T7, am193. This phage, because of the amber mutation it carries, does not make its own T7 RNA polymerase and as a result the phage late genes are not

expressed. All RNA synthesis in the cells infected with am193 is carried out by the *E. coli* DNA dependent RNA synthetase and so it is sensitive to inhibition by rifampicin.

Using this experimental system Marrs and Yanofsky have compared the synthesis and survival of the early T7 messengers and the *E. coli* tryptophan operon messengers in the same cell. In short, they find that whereas the T7 messenger RNAs have half-lives of some 6 to 20 minutes, the tryptophan messenger RNA has a half-life of a mere 80 seconds in the infected cells as it does in normal uninfected *E. coli*.

Marrs and Yanofsky's observation clearly indicates that the enzymatic machinery responsible for degrading messenger RNAs attacks different classes of messengers at different rates; in other words, the degradation machinery must be able to distinguish one messenger from another and any hypothesis seeking to account for the turnover of messengers must be able plausibly to account for this fact.

mechanisms of degradation, and the role of oxygen in this process but, perhaps surprisingly, nothing at all was said of current experimental work on the development of interface barrier coatings. In metal matrix/metal fibre composites the problem of intermetallic formation at the interface, with possible deleterious effects, especially on fatigue properties, is also a serious difficulty. Some elegant experiments on wetting of ceramics by molten metals and alloys were described by Dr J. A. Champion (National Physical Laboratory). These experiments yield the kind of information that designers of composites may need when considering particular combinations of materials for specific purposes.

The only contribution which dealt with the industrial viewpoint—that of the design engineer—was given by Dr M. A. McGuire (GKN Group Technological Centre). He described two case studies in the development of metal matrix composites and outlined a model approach to designing with composites that might be considered as a substitute for conventional materials in simple engineering applications. The general conclusion—not an encouraging one for most of the participants—was that as far as the engineering industry is concerned, metal composites can scarcely hope to compete with metals—and particularly with steels—on economic grounds.

The symposium ended with some interesting ideas put forward by Mr R. W. Jones (Liverpool Polytechnic), who suggested that glass-ceramic fibres might be used as reinforcements for metals. The particular advantages of glass-ceramics over glass are that higher elastic moduli can be obtained; modulus and expansion coefficient can, within limits, be tailored to suit a particular matrix; and they are less susceptible to weakening through surface damage. Difficulties of making good strong fibres are yet to be overcome, however.

## MATERIALS SCIENCE

### Coating Processes

from a Correspondent

THE topic of coatings was considered at this year's autumn meeting of the Materials Science Club held in Great Malvern on November 11 and 12. In an introductory talk Dr J. E. Castle (University of Surrey) described the principal features of surfaces as boundaries between solids, and liquids or gases, and he outlined some recent work on Auger and low energy electron diffraction spectra of surfaces. This work suggests that impurities of surfaces are often different from those in the bulk material and that signifi-

cantly different crystal structures can exist in the near surface regions.

Chemically and electrochemically applied coatings on metals were the subject of talks by Dr C. J. L. Booker (City of London Polytechnic) and Mr R. Walker (University of Surrey). Conversion coatings are essentially methods of thickening or modifying the natural oxide coating of metals, by chemical or anodic treatments, to improve the degree of protection. These are widely applicable to steel, aluminium, magnesium, titanium and their alloys. The simplest treatment is known as the simple dip process which has been developed to produce coatings suitable for painting and as lubricating and corrosion resistant finishes in their own right.

Anodizing is a more sophisticated form of treatment which allows considerable control over the thickness and structure of the film produced, even to the extent of allowing colour decorative effects to be produced by optical interference in thin transparent films of titanium alloys. Electrodeposited coatings are perhaps the most widely applied metal coatings. The Pourbaix diagram provides the useful indication of optimum plating conditions. Acidic baths are usually pre-

ferred, on economic grounds, but in some instances complex electrolytes are required to ensure satisfactory deposits. The important properties in plated coatings are adhesion, porosity, internal stress and brightness, which may be controlled by attention to the preparation of the substrate, and by control of the pH, temperature and current density of the plating solution. In particular, complex additives often have a profound effect on the quality of the deposit. Mr Walker outlined the beneficial effects of benzotriazole on the internal stress and brightness of copper and nickel deposits plated from acidic baths.

Dr R. L. Apps (Cranfield Institute of Technology) described his work on the adhesion of sprayed aluminium coatings on steel and concluded that the grit blasting stage of substrate preparation was all important. Impurities derived from grit debris seem to reduce adhesion, and plasma sprayed coatings were better than those deposited by oxy-acetylene flames. Dr R. F. Smart (Associated Engineering Developments Ltd) also discussed plasma sprayed deposits. The great advantage of this technique is that almost any material can be sprayed and with suitable pre-

## Octanoic Acid and Photosynthesis Inhibition

THERE is a remarkable difference between the *in vivo* and *in vitro* absorption spectra of bacteriochlorophyll *a* (BChl). In organic solvent this pigment has a single far-red absorption band at 770 nm whereas *in vivo* BChl can have several peaks in the 800 to 900 nm region. The spectral shift almost certainly results from the hydrophobic protein environment in which the BChl is sited within the bacterial membranes.

In next Wednesday's issue of *Nature New Biology*, Steffen and Calvin discuss experiments which demonstrate that concentrations of fatty acids which are known to inhibit photosynthesis do so by interacting with the pigment-protein complex responsible for channelling light energy to the reaction centres.

Steffen and Calvin have used chromatophores isolated from the blue-green mutant of *Rhodospseudomonas spheroides* strain R26. This photosynthetic bacterium has a BChl absorption peak at 590 nm and a single far-red peak at 860 nm (B590–860). Treatment of the chromatophore preparations with *n*-octanoic acid at concentrations below  $10^{-2}$  M and at low pH causes the far-red absorption band to shift to 770 nm (B590–770). This shift is not caused by an acid or pH effect because HCl does not bring about a similar

spectral change. With higher concentrations of octanoic acid both the 590 and 860 nm bands are shifted towards the blue with new peaks at 530 and 760 nm (B530–760). The B590–770 resembles the BChl spectrum *in vitro* and B530–760 corresponds to the spectrum of bacteriopheophytin (BChl which has lost its central Mg ion).

There was no evidence that *n*-octanoic acid detaches the pigment from its protein carrier, and, moreover, the absorption shifts were reversed on the removal of octanoic acid. Steffen and Calvin argue that their spectral observations result from the protein-pigment complex unfolding in the presence of fatty acids so as to expose the BChl molecules to the solvent. Measurement of fluorescence depolarization and of light-dark difference spectra showed that this unfolding decreases the efficiency of energy transfer between the light harvesting BChl molecules resulting in an inhibition of reaction centre (P890) oxidation.

NMR studies indicate that octanoic acid molecules interact weakly with the chromatophore membrane and that the polar carboxyl group is not involved in the binding. Steffen and Calvin in fact have additional evidence for hydrophobic interaction because the neutral ester methyloctanoate can act in the same way as octanoic acid.



cautions pore free deposits, up to several hundred microns in thickness, may be produced. Principal applications are chemical and wear resistant coatings, reclamation, and thick film electronic circuitry.

Three other high temperature coating processes discussed were vacuum coating, diffusion coatings and pyrolytic coatings. Dr L. Holland (Edwards High Vacuum Ltd) described the principal vacuum coating techniques with particular reference to R. F. Sputtering which has proved to be a most versatile method. He outlined the range of materials which can be processed and the quality of the deposits which can be produced. Pyrolytic coatings of refractory metals may be deposited by chemical or thermal breakdown of their volatile halides, and carbon and silicon carbide from hydrocarbon and silane gases respectively. Dr E. H. Voice (OECD Dragon Project) described the Dragon nuclear fuel pellet as an example of the use of the technique. The pellet consists of a sphere of uranium oxide or carbide coated with two layers of pyrocarbon interspersed with one of silicon carbide; the total diameter is about 1 mm. The coatings are applied by a fluidized bed technique. Dr N. A. Lockington (Chrome Alloying Co. Ltd) devoted most of his talk to the chromizing process, in which a chromium rich layer is diffused into the surface of the ferrous material or nickel alloy. The most advantageous method is to use a pack process; the components are packed in an inert medium (alumina) containing ferrochrome and a halide. On heating, the halide acts as a transport medium to move chromium from the ferrochrome to the component. Several other processes operating on the same principle are feasible but apart from siliconizing, which is used to produce disilicide coatings on superalloys, they are not important commercially.

Non-metallic coatings discussed included paints and plastic coatings on metals. Dr L. H. O'Neill (Paint and Varnish Manufacturers Research Association) described several new developments in paint finishes, including thermosetting acrylic finishes which should prove to be more durable than thermoplastic acrylic lacquers now widely used as car finishes. Domestic paints containing both urethane and silicones which combined fast drying and non-drip qualities with good durability are also being developed. Dr O'Neill also discussed an intriguing development which aims to replace a conventional white pigment by a suspension of bubbles to produce an opaque finish, but these finishes have so far proved to be too soft for commercial use. Plastic coatings may be applied to metals by dipping the preheated metal part into either a fluidized bed of polymer

powder or into a liquid plastisol. The former process is applicable to polyethylene, nylon and unplasticized PVC, whereas the latter is applicable only to plasticized PVC compounds. Dr G. E. Barrett (Plastic Coatings Ltd) described the precautions necessary to obtain good adhesion and satisfactory coatings. He also discussed the requirements of the polymer to render it suitable for the process. These requirements are very severe and virtually limit the fluidized bed process to low density polyethylene and nylons 11 and 12, which have the necessary wide melting range and easy flow properties without degradation.

Dr A. Rantell (Polytechnic of the South Bank) spoke on metal plated coatings on plastics. These coatings are produced by a conventional electroplating technique preceded by etching and chemical deposition stages. The essential pretreatment is the deposition of a minute quantity of palladium on to the plastics surface. This acts as a catalyst for an electroless nickel plating process which in turn provides a substrate for the conventional electroplate. Adhesion is controlled by an initial etching process. Experience has shown the most

suitable substrate material to be ABS, the rubber phase of which may be etched out to provide an excellent mechanical key for the plate. More recently, techniques have been developed for plating polypropylene and nylon with satisfactory results, the basic problem being to provide a suitable surface at the initial etching stage.

Dr D. White (Houldsworth School of Applied Sciences, University of Leeds) described coatings formed by dipping metal articles into suspensions of clay minerals. Thin coatings of montmorillonite have been found to give excellent protection against oxidation on steels, and titanium alloys. This is sufficient to allow them to be heat treated at temperatures up to 750°C without other forms of protection. The exact mechanism of protection is not clear but it is known that very thin films (about 1  $\mu\text{m}$ ) are effective. These coatings are very cheap and easy to apply.

At the meeting the A. A. Griffith silver medal for 1971 was presented to Professor J. White (University of Sheffield) and the Materials Science Club Prize was presented to Mr S. Turner (ICI Ltd, Plastics Division).

## Evolutionary Behaviour of Quasars

IN attempting to fit the observational data of quasars at radio and optical wavelengths to a reasonable cosmological model, it is necessary to assume some form of evolutionary behaviour of the sources. Different workers have, however, disagreed about the precise nature of this evolution. For instance, Schmidt has claimed that only the population density of quasars is time dependent whereas the luminosities are constant, and Rowan-Robinson maintains that either density or luminosity evolution is present, but in both optical and radio properties. On the other hand, Arakelian has claimed not only that density evolution is absent, but that the optical and radio properties evolve differently.

The different conclusions of these workers are a consequence of their different ways of analysing the data (chiefly from the 3C catalogue). Rowan-Robinson has used the luminosity-volume test in which the numbers of sources in a given range of radio or optical luminosity are counted for different redshifts. Schmidt's procedure was to calculate the average value of a ratio of volumes, one corresponding to the redshift of a particular source, the other corresponding to the redshift at which such a source would disappear at the survey limit. A uniform distribution will give a value 0.5.

The new feature introduced by Arakelian was to treat the optical and radio data separately. Using Schmidt's data and method as well as the 4C catalogue he found only radio (but not optical) luminosity evolution.

Arakelian's results have been subject to some doubt, and a reconciliation of the conflicting conclusions is presented in an article in next Monday's *Nature Physical Science* by L. M. Golden of the University of California. Golden points out that Arakelian's work suffers from a selection effect. Thus for a representative radio (optical) sample, those of low optical (radio) luminosity will be observed only at small redshifts. Arakelian has apparently also used a non-representative sample, and so Golden has repeated Arakelian's analysis, correcting for these effects. For this Golden essentially uses only the bright sources, and assumes an Einstein-de Sitter cosmology (although the conclusion is insensitive to the value of the deceleration parameter). The result is that the radio/optical luminosity ratio is independent of redshift for a least squares fit, indicating similar evolution at both wavelengths.

Using this result, Golden also points out that Schmidt's analysis in fact permits both luminosity and density evolution. In this way the results of all three procedures can be reconciled.

# Acoustics of Insect Song

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**This article describes the state of knowledge about insect sound production and hearing with special reference to *Drosophila* and some larger insects.**

Most animals communicate acoustically using a transducer which causes vibrations in the medium around them. These vibrations can be received at a distance by a vibration sensitive receptor on another animal. By producing a patterned train of vibrations, the animal producing the sound can provide a species specific or a context specific signal which may be important in the sexual or social behaviour of the animal.

Other aspects of such songs may be of considerable importance to the animal because of the limited bandwidth available, for example one species tends to hear the songs of another. Also, in the case of a small animal, the range of the songs may be small and so it may be advantageous to produce a directional sound output and one that is adapted to the acoustic receptor. But the complexity of vertebrate songs and of the sound producing and receiving mechanisms means that it is difficult to study the acoustics rigorously.

Many insects, however, produce songs comprising nearly pure tones and do so by a mechanism which can be excited artificially and described in physical terms. For these reasons, the study of insect acoustics is informative. But from the biologists' standpoint it has another interest because insects are small animals and communicate with sounds whose wavelength may be many times the length of their bodies; in these conditions the acoustic power can be minute and the song may have a degree of intimacy unknown in vertebrates.

## Sound Waves

Many of the problems encountered in insect song have been examined either theoretically<sup>1</sup> or in the context of acoustical engineering<sup>2</sup>. Sound is propagated as a series of waves of compression and rarefaction; there is little absorption of energy and so power is radiated over a considerable distance. If acoustic power radiates from a point into a free space, a spherical wave is propagated and the acoustic power at a distance decreases as the square of the distance from the source. Sound intensity, the power per unit area, measured normal to the source is the product of the pressure and the particle velocity in the wave<sup>2</sup>.

In acoustics it is often convenient to consider the specific resistance of the medium, expressed in  $\text{kg m}^{-2} \text{s}^{-1}$ , which is the product of the density of the medium,  $\rho$ , and the velocity of sound,  $c$ . In a manner analogous to electrical power, acoustic power can also be considered in terms of the sound pressure or of the particle velocity. The power per unit area is given by

$$I = pu = u^2 \rho c = \frac{p^2}{\rho c} \text{ W m}^{-2} \quad (1)$$

where  $p$  is the pressure in  $\text{N m}^{-2}$  and  $u$  is the particle velocity in  $\text{m s}^{-1}$ . Sound power is often measured in decibels (dB) where

0 dB =  $10^{-12} \text{ W m}^{-2}$ . This is equivalent to a pressure of  $2 \times 10^{-5} \text{ N m}^{-2}$  or a particle velocity in a plane wave of  $0.5 \times 10^{-7} \text{ m s}^{-1}$ .

As with electrical power, this relationship only holds if the pressure and velocity terms are in phase. In a plane wave, or at a large distance from a point source, pressure and particle velocity are in phase, but as the distance from the source decreases to about the wavelength of the sound, the divergence of the waves must be considered. In this condition, the spherical wave, the particle velocity and the pressure are no longer in phase<sup>2</sup> and the phase angle can be obtained from

$$\text{Tangent phase angle} = \frac{\lambda}{2\pi r} \quad (2)$$

where  $\lambda$  is the wavelength and  $r$  is the distance from the source. Thus the phase angle between the pressure and the particle velocity increases towards  $90^\circ$  as the source is approached; this has important consequences when small sources are considered.

The acoustic power from a point source in free space varies inversely with the square of the distance and the pressure varies inversely with the distance from the source. Because the power is the product of the in-phase pressure and particle velocity, the relative magnitude of the particle velocity increases more rapidly than the pressure close to the source according to

$$u = \frac{p}{\rho c} \sqrt{1 + \left(\frac{c}{2\pi fr}\right)^2} \quad (3)$$

where  $f$  is the sound frequency. At a distance less than about  $1/6$  wavelength from the source, the particle velocity varies as  $r^{-1}$ .

As the particle velocity and pressure are not in phase, the air is behaving reactively close to a point source. This is of practical importance with small sources where most of the air load is reactive; for high acoustic efficiency, the source must be large enough for the loading to be resistive and this will be found only with sources whose diameter exceeds  $\lambda/\pi$  or about  $1/3$  wavelength (Fig. 1).

Below this size, the acoustic loading depends on whether the source is freely suspended or set in a baffle. For the freely suspended piston source, the resistance falls as radius<sup>-4</sup> and with the piston in a baffle the resistance falls as radius<sup>-2</sup>. Thus it is an advantage from the point of view of sound production efficiency if a source is placed in a baffle whose diameter exceeds  $1/3\lambda$ .

If the diameter of a flat piston source is larger than about  $\lambda/\pi$  the sound is radiated as a beam which becomes longer and narrower as the diameter of the disk increases. A freely suspended piston smaller than  $\lambda/\pi$  in diameter radiates from both sides and the sound distribution is a figure-of-eight whose long axis is normal to the disk. If the disk is surrounded by a baffle, the sound distribution on each side of the baffle is hemispherical with the disk at the centre.

It is evident from the critical examinations which have been made of several types of insect song that the types of transducers and the directional properties are closely related to the biological context in which they are used.



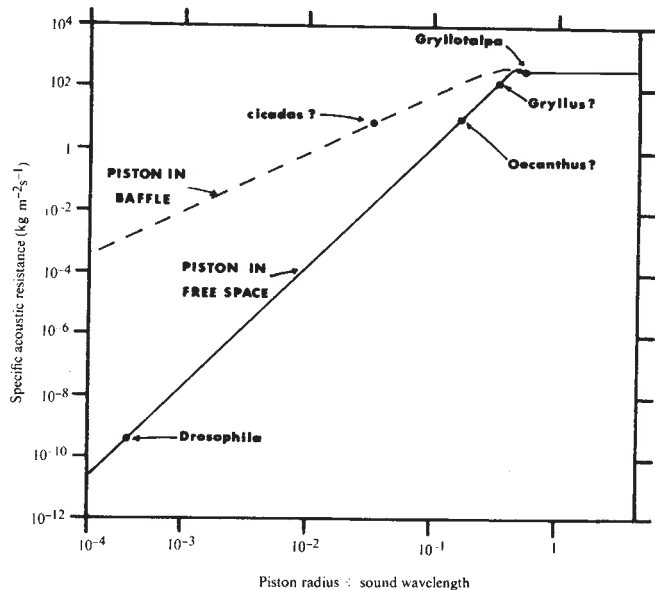


Fig. 1 Graph of the specific acoustic resistance against radius for two types of source. The curves are redrawn from ref. 2 and the points for *Drosophila* and *Gryllotalpa* are calculated from my data<sup>12,26</sup>; those for cicadas, *Oecanthus* and *Gryllus* are estimated from the size and type of transducer.

## Insects' Hearing

Insects have many different types of auditory organ<sup>3,4</sup>. The simplest of these is the sensory hair which responds to displacement of the surrounding air. These receptors are found on the anal cerci and on other parts of the body and they have a low frequency response, giving a synchronous nervous discharge at frequencies up to about 300 Hz (ref. 5).

The antennae of many insects also respond to sound. In *Diptera*, the antennae are the principal organs of hearing and are tuned and directional. In the male mosquito, the antenna is most sensitive at the wing-beat frequency of the female<sup>6,7</sup> and, because the antenna does not vibrate along its axis when exposed to sound arriving from the side, indicates the direction of the source. In cyclorrhaphous flies the antenna bears a feathery arista which projects sideways from the apical joint; the arista can be displaced by the steady air flow or by the sound waves produced by the wings<sup>8</sup>. In *Drosophila*, the antennal arista can be set in vibration by sounds of 180 to 220 Hz and I have found that the amplitude of vibration is greatest when sounds arrive from 90° to the plane of the arista—about 30° away from the sagittal axis. There is mechanical and physiological evidence that the excitation of these receptors is by the particle displacement in the sound wave.

Higher frequencies can be received by tympanal organs; these are found in many groups, such as *Orthoptera*, *Homoptera* and *Lepidoptera*. There is a thin tympanal membrane on the body surface which is closely associated with internal tracheae or air sacs. Sensillae may be attached to the tympanal membrane or to the tracheae. The frequency response is usually from a few hundred Hz to more than 60 kHz and the nervous response is an asynchronous firing at the start or at the end of the sound pulse.

Frequency response curves for this type of receptor often show peak sensitivity to the song of the same species<sup>9,10</sup>, and this may be due to excitation of specific neurones situated at antinodes of vibration of the tympanic membrane<sup>11</sup>; these different receptors in the locust are tuned with a  $Q$  of 3 to 6 and are thus able to discriminate between different frequencies and detect sound pulses of brief duration. Unfortunately no studies have yet been made of the mechanics of the tympanal organ in those insects that produce very pure single tones.

At one time it was thought that all insect hearing made use of displacement receptors. This arose because it was considered that the air sacs separating the two tympanal diaphragms were

acoustically transparent. This view was reinforced by measurements which have the typical figure-of-eight directional response curve for the locust<sup>3</sup>; this was, however, probably produced with a mutilated insect. Michelsen<sup>11</sup> has recently examined the acoustics of the ear and air sacs in locusts and has shown that at the lowest frequencies, up to 5 kHz, the air sacs are effectively transparent and the response is probably to particle displacement. At higher frequencies from 5 to 15 kHz the impedance of the air sacs rises so the response is to the sound pressure; and because the body is of a substantial size, diffraction causes hearing to be unidirectional. At frequencies greater than 15 kHz, the acoustic load on the tympanum becomes significant and damps the natural resonances of the diaphragm; hearing is therefore not frequency sensitive.

The absolute sensitivity of hearing varies greatly, both with the insect and with the mechanism. Early work suggests thresholds of 50 to 70 dB for many species, including the locust<sup>10</sup>; these were measured on the basis of an obvious visible or audible record of the nervous discharge. If the threshold is defined in terms of the spontaneous nervous activity, during which the response is significantly greater over a period of time than the spontaneous response, it can be shown that the threshold level in locusts is some 40 dB less than the "all-or-nothing" threshold<sup>11</sup>. This absolute threshold may not be biologically significant because the duration of a steady state sound must be long if it is to be detected; most insect songs are made up of short pulses and can be recognized after as few as three pulses (A. W. Ewing and H. C. B.-C., to be published).

It appears that hearing in insects may discriminate the frequency, the intensity and the direction of the sound.

## Very Small Insects

Many small insects such as some wasps, beetles and bugs produce sounds which are usually rather quiet. The general acoustic situation in such small animals can be analysed by using as an example the courtship song of *Drosophila melanogaster*—the waveform is fairly simple and the dimensions and amplitude of vibration of the sound producing organ are known<sup>12</sup>.

During courtship, male *Drosophila* extend one wing towards the female and vibrate it, thus producing a series of sound pulses<sup>13–15</sup>. Each pulse is produced by a single beat of the wing which has a peak to peak amplitude of about 1 mm and period of about 6 ms (ref. 12). The wing is elliptical and has an area of about 1.25 mm<sup>2</sup>. For the purposes of calculation of the acoustic power produced, the wing can be treated as a doublet source and the power is given by<sup>2</sup>

$$I = (2\pi fd)^2 r_M \quad (4)$$

where  $I$  is the r.m.s. power in W,  $f$  is the frequency in Hz,  $d$  is the displacement in m and  $r_M$  is the acoustic resistance of the source in kg s<sup>-1</sup>. For *Drosophila*,  $f$  is 166 Hz,  $d$  is  $5 \times 10^{-4}$  m, and  $r_M$  is the product of the area of one side of the wing,  $1.25 \times 10^{-6}$  m<sup>2</sup>, and the specific resistance of a source whose effective radius is  $3.0 \times 10^{-4}$  times the wavelength of the sound (a resistance of  $4 \times 10^{-10}$  kg m<sup>-2</sup> s<sup>-1</sup> (Fig. 1)). The power radiated by each side of the wing is  $1.3 \times 10^{-16}$  W, which is too little to be heard directly.

The song must be related to the acoustics of the receptor which responds to particle displacement and is roughly tuned to the song frequency. During courtship, the male stands less than 5 mm (about 1/400 the sound wavelength) from the female fly. Sound pressure is distributed in a figure-of-eight pattern around a doublet source (Fig. 2) and, at a distance  $r$ , the isobar has an area  $\pi r^2$ . If the inverse square law was obeyed, as it is at values of  $r$  greater than about  $1/3\lambda$ , the sound pressure would be  $1.7 \times 10^{-12}$  W m<sup>-2</sup> or 2.5 dB. With a doublet source at a distance  $r$  less than about  $1/6\lambda$ , the pressure is a factor of

$$\frac{\lambda}{2\pi r}$$

greater than this and the particle velocity is increased by a factor

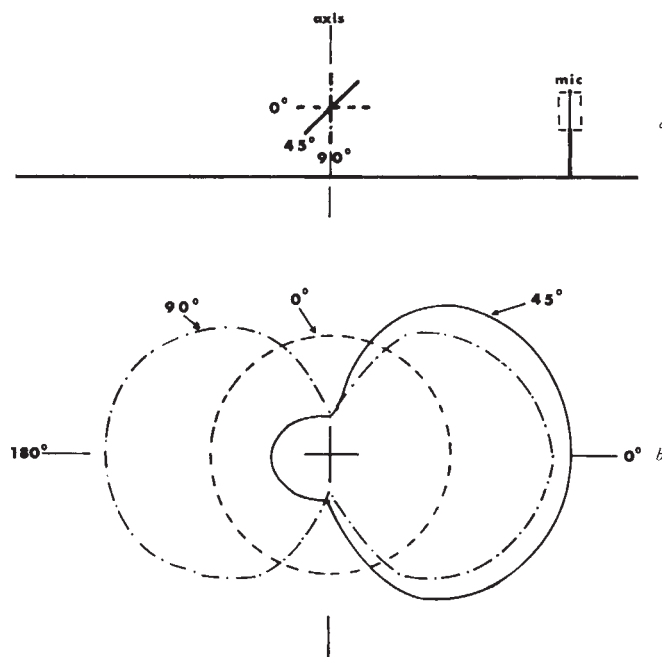
$$\left(\frac{\lambda}{2\pi r}\right)^2$$

(ref. 2) which at 5 mm is 36 dB and 72 dB louder respectively. At 2.5 mm, which is rather more than the length of the fly, the calculated pressure is 51 dB above  $2 \times 10^{-5} \text{ N m}^{-2}$  and the particle velocity is 95 dB above  $0.5 \times 10^{-7} \text{ m s}^{-1}$  (Fig. 3.)

If a pressure sensitive microphone is used to record *Drosophila* song, elaborate sound insulation and high gain amplifiers are necessary<sup>15</sup>. But with a velocity sensitive microphone, no special sound insulation is needed in a noisy laboratory, and in experiments with artificial songs Ewing and I used pulses of 107 dB because lower powers were less reliable<sup>17</sup>.

Flies stand on flat surfaces to court and the male fly faces the female. The courtship wingbeat is at  $45^\circ$  to the substrate<sup>12</sup> and this can be modelled using a small loudspeaker set close to a flat surface (Fig. 2). When the diaphragm is vertical, the distribution is the expected figure-of-eight. When the diaphragm is horizontal, the sound is quieter and non-directional in the plane of the surface. If the diaphragm is at  $45^\circ$ , the output is directed over an arc of  $90^\circ$  in the direction of squish; as the male fly orients himself roughly facing the female, he directs his song at her and other flies in the vicinity will not receive his song. This is important for a genus which tends to congregate on fruit or flowers.

From this study of *Drosophila*, it is clear that the displacement receptor is an advantage to animals which are very much smaller than the sound wavelength. Fish, for example, live in a medium with a high propagation velocity,  $1,430 \text{ m s}^{-1}$ , so the acoustic wavelength of their swimming or that of their food is many times the body length. Displacement receptors are used to monitor swimming or to catch food<sup>18,19</sup> but, at higher frequencies or over longer distances, fish use pressure receptors;

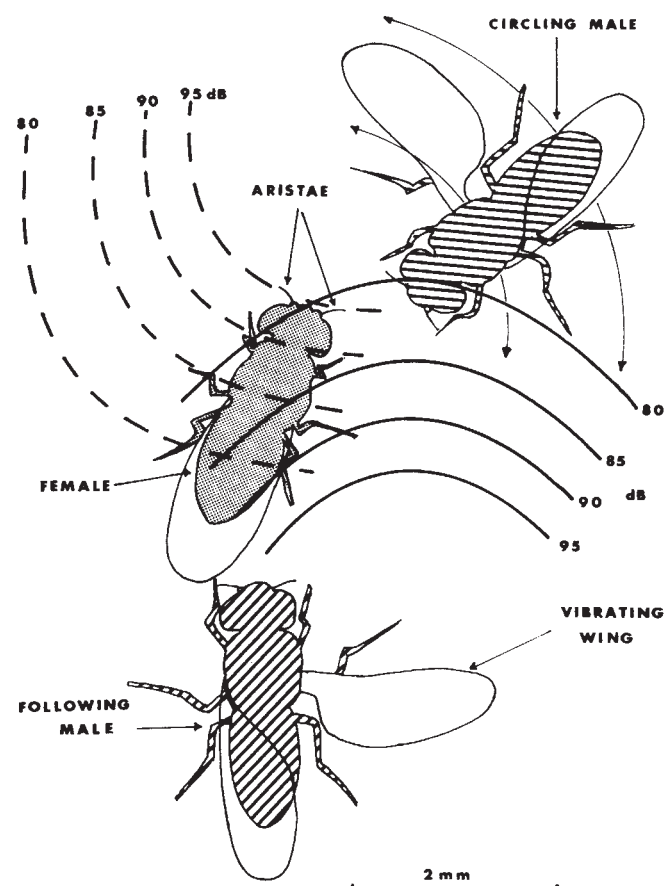


**Fig. 2** Diagram of an acoustic model of *Drosophila* and its directional properties. *a*, A 50 mm diameter loudspeaker radiating 150 Hz is set 50 mm above a flat surface and 150 mm from a microphone, *mic*. The loudspeaker can be rotated about a vertical axis and the angle of the diaphragm to the surface can be altered. At 150 Hz, the sound wavelength is 2.25 m. *b*, Polar graph showing the relative sound pressure for different angles of the loudspeaker diaphragm to the surface. — · — · —, The diaphragm is vertical and the output is a figure-of-eight either side of the diaphragm; — — —, the diaphragm is horizontal and the output is non-directional; — — —, the diaphragm is at  $45^\circ$  and the output is directed forwards and is maximal with the microphone in position *a*.

moths also use these in the same way to hear the sonar cries of bats<sup>20</sup>.

## Large Insects

Many insects generate songs of remarkable noisiness. To produce these with maximum efficiency, a radiating surface with a radius larger than  $1/6 \lambda$  is desirable and this restricts the frequency to more than about 3 kHz; a large number of insects, however, produce songs at more than 20 kHz. The songs of many insects have a dominant frequency and the energy spectrum is often narrow (Fig. 4); this suggests that the frequency is controlled by a part of the system that is not highly damped by the acoustic load and, because damping implies high conversion efficiency, the conversion efficiency may be low. Nevertheless, some of the loudest insects produce very pure songs.



**Fig. 3** Diagram showing the sound particle velocity produced by a male *Drosophila melanogaster* courting a female fly. The particle velocity contours are calculated and expressed in dB above  $0.5 \times 10^{-7} \text{ m s}^{-1}$ .

## Frequency Control

In most cases, the song frequency is determined by the resonance of either a disk or an elastic bar. The tymbal of cicadas<sup>21</sup> and the harp of field crickets<sup>22</sup> act as clamped disks excited in the case of cicadas by the periodic contraction of the tymbal muscles and in crickets by the passage of a plectrum over the file. The  $Q$  of the undamped disk of a cricket is about 25 but the mass of the air load damps this somewhat and the natural song has a bandwidth that suggests a  $Q$  of about 10 (Fig. 4).

The elastic bar is found in tettigonioids and mole crickets and vibrates at a frequency proportional to  $\text{length}^{-2}$  (refs. 23, 24). The resonance is excited by the passage of a plectrum over a toothed file but the type of song depends on whether the drop of the teeth on the file is synchronous with the primary reso-



nance of the elastic bar. In most bush crickets, the resonance frequency is more than 10 kHz, and the closing stroke of the wings causes the plectrum to pass over 20 to 60 of the teeth on the file; at each drop of the tooth, a damped resonance of the bar occurs<sup>25</sup> and so the song is modulated first at the wing closing frequency and then at the tooth drop frequency. This type of song is usually impure, with a system  $Q$  of less than 5 and a broad frequency-energy spectrum (for example, *Phlugis* in Fig. 4).

If the bar is excited synchronously, the resonance is maintained and the frequency is very much purer (*Gryllotalpa* and *Drepanoxiphus*, Fig. 4); the song is modulated at the wing-closing frequency (up to 140 per second<sup>24</sup>). In this type of song the system  $Q$  is usually higher and the carrier frequency may be characteristic of the species and the range of individual variation; the bandwidth of an individual's song<sup>26,27</sup> suggests that the song carrier frequency may be of considerable biological importance.

The air load causes considerable damping of the vibration of the wing of a mole cricket and the song decays rapidly at the end of the pulse, suggesting a low  $Q$  resonator. The song is much purer than would be expected from such a system and this may be due to the considerable inertia of the dorsal field of the wing which will tend to cause the plectrum to drop the teeth on the file at a fairly constant rate; it is clear from records of the song at low temperature that the resonance of the elastic bar can be excited asynchronously and that the song is then far less pure<sup>26</sup>. Similarly, the examination of several tettigonioids suggests that the effect of altering the inertia of the wing<sup>28</sup> or of driving the wing at a rate not related to the song frequency is to produce an erratic pulse<sup>29</sup>.

In summary, the song frequencies are determined by the excitation of some mechanical resonance but the purity of the song and the form of the pulse envelope may be affected by the way in which this excitation occurs.

## Energetics of Singing

Acoustic measurements are difficult to make with precision and the fact that most insects do not remain stationary only complicates the problem. Mole crickets which sing from a burrow (Fig. 5) produce a pure song very regularly for a long period and it is therefore possible to measure the whole sound

field and hence the total sound output. For *Gryllotalpa vineae* the output is about 1.2 mW with a peak level at 1 m of more than 90 dB. As the muscles responsible for the song only weigh about 50 mg and produce an estimated 3.5 mW, the efficiency is very high (about 35%, ref. 26). The small wings are placed across the horn-shaped burrow (Fig. 5) which provides a resistive load at the song frequency of 3.5 kHz. Examination of the size of the teeth and the area of the vibrating surface, the harp, suggests that the harp vibrates through about 125  $\mu$ m.

If the harp is resistively loaded, the air mass can be calculated from

$$M_a = \frac{R_m}{2\pi f} \quad (5)$$

where  $M_a$  is the air mass in kg,  $R_m$  is the acoustic resistance of the source in  $\text{kg s}^{-1}$  and  $f$  is the frequency in Hz. The area of the harp is 13  $\text{mm}^2$  and the air mass is about 0.25 mg. The mass of the membrane of the harp is about 0.35 mg and that of the veins surrounding the harp 0.8 mg, so the mass of the membrane and its air load are similar to that of the veins that drive it, which suggests a  $Q$  of less than 2. The mass of the dorsal field, however, is about 5 mg and, if this is considered to be the mass driving the resonant system, the system  $Q$  is given by

$$Q = \frac{\text{driving mass}}{\text{air load} + \text{membrane mass}}$$

This yields a value of 8.5. Considering the mass of the whole wing (12 mg)  $Q$  is 20. The measured  $Q$  is, in fact, about 16 (Fig. 4) so this explanation of the mechanism is a possible one. It would be useful to know the effects of altering the inertia of the wings or the compliance of the various parts of the system.

Although the energetics of *Gryllotalpa* song are probably the simplest to examine, other systems can also be studied; the tree crickets of the Oecanthinae produce very pure songs at about 2 to 3 kHz and hold the wings vertical while singing, making a doublet source. Pasquinely and Busnel's measurements of the sound level of 68 dB at 10 cm (ref. 25) give a total power of  $1.2 \times 10^{-7}$  W. In this genus the area of the harp is about 15  $\text{mm}^2$  for both wings so the power output for a radiator of this size

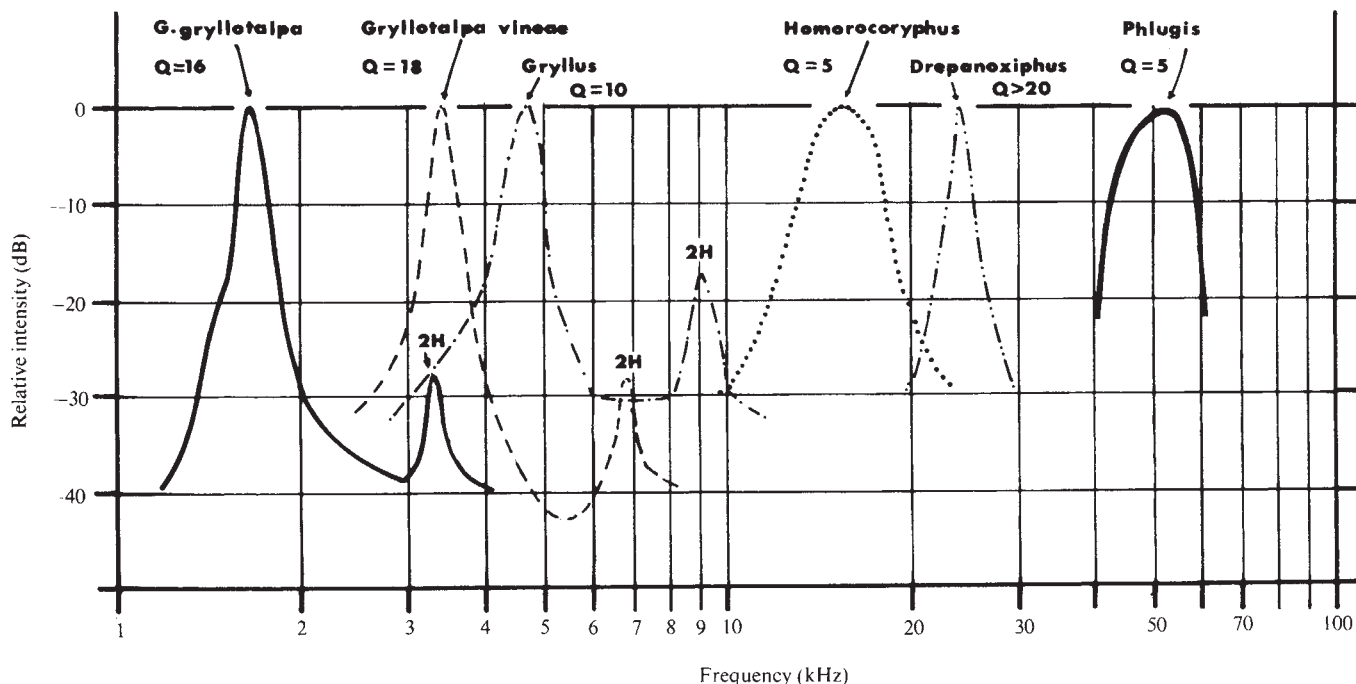


Fig. 4 Frequency-power spectra for various insects expressed as dB below peak level. The curves show the second harmonic content, 2H, and the  $Q$  has been calculated from the bandwidth at 3 dB below the peak. Data for *Gryllotalpa* and *Gryllus* are original, for *Homorocoryphus* from Bailey<sup>24</sup>, for *Drepanoxiphus* and *Phlugis* from Suga<sup>28</sup>.

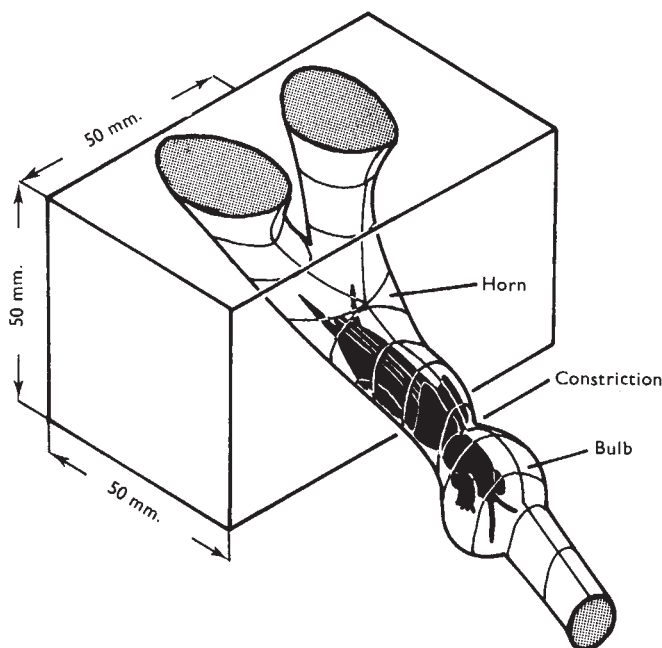


Fig. 5 Isometric drawing of the horn shaped burrow of *Gryllotalpa vineae*<sup>26</sup>. The horn provides a resistive load to the small wings of the insect, the bulb tunes the horn throat reactance and the two openings at the surface produce a disk shaped beam of sound.

(Fig. 1) is produced by a vibration of amplitude 7.5 to 10  $\mu\text{m}$  (equation (4)) which is compatible with the dimensions of the file. The sound power is 40 dB less than that of *Gryllotalpa vineae* and Morse<sup>30</sup> has written of it that "if moonlight could be heard, it would sound like that". The biological value of such a low song intensity is not clear but the wavelength, about 15 cm, is less likely to be diffracted by such things as leaves than the 1 to 3 cm sound waves of bush crickets and so direction finding at close range may be better.

Field crickets of the Gryllinae make far louder sounds than Oecanthinae and a power output of about  $6 \times 10^{-5}$  W has been estimated<sup>26</sup>. The song wavelength is about half that in *Oecanthus* so the specific radiation resistance is about 16 times greater and, because the lateral fields of the wings are held against the side of the body, the effective diameter of the source is much increased. The result is that the radiation resistance is many times that in *Oecanthus* but still far less than that in *Gryllotalpa* in its burrow. The efficiency of sound production is estimated at about 5%. Acoustically, cicadas can be regarded as a pair of small transducers in an infinite baffle<sup>31</sup>. The resonance of the tymbal is not very highly damped (though this has not been measured) and this is consistent with a system in which the acoustic loading is not very high (Fig. 1).

It should be possible to offer a fairly precise description of the acoustic transducer but so far this has only been done in the simplest cases.

## Output Directionality

In most biological contexts it is an advantage to direct the sound output. For a given sound output, a directional beam will have a greater range than the same power distributed equally in all directions.

If the sound is produced by a displacement transducer or a doublet source, the output is a figure-of-eight (Fig. 2) but the distribution is affected by proximity to the substrate and the form of the transducer. Nöcke has shown that *Gryllus* sound is produced from a modified doublet source and that the sound is loud behind the insect but that there is a null at right angles to the sagittal plane<sup>22</sup>. This directionality is partly due to the angle at which the wings are held and partly to the substrate;

the effect is that the insect can turn and direct a beam of sound close to ground level. Similar effects probably occur with *Ephippiger* but here the song is directed backwards and to one side by the pronotum and abdomen<sup>25</sup>.

With a small pressure transducer, the sound output is non-directional but the shape of the surroundings may be important. Aidley has shown that cicadas produce a maximum intensity either side of the sagittal plane which can be related to the diffraction of sound by the body of the insect and the position of the tymbals<sup>31</sup>. In mole crickets, the horn can be regarded as a large pressure transducer set in a flat baffle and has two mouths each about  $1/3 \lambda$  in diameter and about  $1/3 \lambda$  apart. The calculated response for a source of size about  $0.6 \lambda$  by  $0.3 \lambda$  is a semicircular disk shaped beam normal to the long axis of the source and the measured sound distribution is similar to this<sup>26</sup>. The advantage is that the surface area of such a beam viewed parallel to the ground is larger than that of a hemisphere of the same total surface area and this would be about 1.4 times as effective in attracting horizontally flying insects as the same power radiated equally in all directions.

## The Complete System

Although much is known of the behavioural context of different types of songs in many different species, very few studies have been made of the acoustics of both the sound producing and sound receiving organs in the species and these have not so far been sufficiently rigorous. Little, for example, is known of the directional properties and acoustics of field cricket hearing, nor is much known of the acoustics of sound production in locusts. In my speculative examination of the acoustics of *Drosophila*, I have endeavoured to show that it is only valid biologically to consider sound production as it is related to the context and the acoustics of each end of the system.

I thank Dr G. T. R. Done for assistance with some of the mechanics and Dr H. Nöcke for discussing his work before its recent publication. I have not, at the time of writing, been able to obtain his paper.

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# Absolute Gamma-ray Dosimetry by Recoil Electron Spectroscopy

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**The difficulties associated with the measurement of continuous gamma-ray spectra can be overcome by a new absolute dosimetry method which does not require the use of other detectors or of radiation standards.**

We have developed a new method of absolute gamma-ray dosimetry which is based on recent work in Compton recoil gamma-ray spectroscopy<sup>1,2</sup> and which properly accounts for the most fundamental quantity in the specification of a gamma-radiation field, namely the absolute gamma-ray energy spectrum. The sensitivity of the method is of particular importance in radiation biology and environmental science<sup>3-4</sup>.

## Continuum Spectroscopy

The large number of types of gamma-ray spectrometers that already exist determine gamma-ray energies of discrete line spectra. But the inability of these spectrometers to distinguish easily between photoelectric and Compton scattering events precludes measurement of continuous spectra. Suppression of Compton scattering requires detectors whose size and/or density perturbs the gamma-ray spectrum because of scattering in the detector material itself. In detectors of low atomic number, however, Compton scattering predominates for a broad gamma-ray energy range. For example, in lithium-drifted silicon detectors, which have been used in Compton-recoil gamma-ray spectroscopy, Compton scattering dominates all other types of interactions for gamma-ray energies from 0.2 MeV to ~3.0 MeV. For plastic and liquid scintillators such as stilbene and xylene, Compton scattering dominates over an even greater energy region—from 0.02 MeV to ~5.0 MeV.

Although the correspondence between the gamma-ray energy spectrum and the resultant Compton electron energy spectrum is not uniquely one-to-one, it can be represented by

$$W(E) = N_e \int \frac{d\sigma_c}{dE}(E, \epsilon) \Phi(\epsilon) d\epsilon \quad (1)$$

where  $W(E)$  is the Compton distribution of recoil electrons per unit time and  $\Phi(\epsilon)$  is the gamma flux energy spectrum. The kernel

$$\frac{d\sigma_c}{dE}(E, \epsilon)$$

of this integral equation is the Compton cross-section per free electron, scaled by  $N_e$ , the total number of electrons in the detector. The Compton cross-section may be obtained from the Klein-Nishina formula and equation (1) may then be unfolded to determine the gamma-ray energy spectrum,  $\Phi(\epsilon)$ .

We have used a lithium-drifted silicon detector 2.0 cm<sup>2</sup> by 0.5 cm depletion depth, mounted in a light aluminium vacuum chamber, to measure the induced Compton recoil electron spectra of unknown gamma radiation fields. The detector assembly produces very little perturbation of the gamma-ray spectrum and output pulses due to Compton electrons that are totally absorbed within the detector are sorted and the information stored in a multi-channel analyser; pulses caused by Compton electrons that enter or leave the detector are electronically rejected. These so called wall events interact in the dead regions of the solid state detector, where the weak fringing field produces relatively slow charge collection. Consequently, wall events in these solid-state detectors possess much slower rise times and can therefore be rejected using pulse-shape discrimination<sup>1</sup>. Finite detector size corrections are used but these become unreliable at about 2.2 MeV for this particular silicon detector.

Now  $W(E)/N_e$  represents the  $\Phi(\epsilon)$  induced Compton electron distribution per free electron within the silicon detector and, within other materials of low atomic number, an identical Compton electron distribution per free electron will be induced by  $\Phi(\epsilon)$  provided that the amount of material does not perturb the gamma-ray spectrum and that electronic equilibrium has been established within the material (thus precluding from consideration a surface region of thickness equal to the range of the Compton electrons). The fundamental quantity,  $W_{1M}(E) = W(E)/N_e$ , will therefore be referred to as the infinite medium Compton electron spectrum per free electron.

## Scintillators

Many of the same considerations which have been applied to the lithium-drifted silicon detector can also be applied to the use of conventional liquid and plastic scintillators in which Compton response predominates. The rejection of defective events caused by recoil electrons which leave the scintillator is, however, not easily amenable to experimental treatment as it is in the silicon detector. There is at least one alternative solution to the problem of electronic non-equilibrium. Because the problem is essentially a surface effect, it can be minimized by choosing larger scintillators with higher volume-to-surface ratios; increasing the scintillator size, however, increases the probability of multiple Compton interactions. This new problem can be viewed in several ways. The presence of the

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larger detector perturbs the gamma-ray spectrum such that the gamma field inside the detector is quite different from the external field. If each individual Compton interaction could be detected by an individual scintillation, the resultant Compton electron spectrum could be unfolded by the simple application of equation (1) to establish the average internal gamma-ray energy spectrum. Unfortunately it is not possible to separate individual scintillations due to multiple Compton interactions. Thus, for example, a multiply scattered 4.0 MeV gamma-ray that successively induces two Compton-recoil electrons of 1.0 MeV and 2.0 MeV in a scintillator will produce only a single response corresponding to an energy deposition of roughly 3.0 MeV. By contrast with solid state silicon detectors, which require finite size corrections, one must consider multiple scattering effects in continuous gamma-ray spectroscopy with large scintillators. And by analogy with the treatment of finite size effects, a correction factor would be introduced into the kernel of equation (1). This factor can be experimentally determined through the use of mono-energetic gamma-ray sources or it can be calculated by Monte Carlo methods. Once this multiple scattering factor is established for a particular scintillator, equation (1) can be used to determine the external gamma-ray energy spectrum.

On the basis of this work, it may also be possible to determine the internal gamma-ray spectrum, that is, the spectrum which exists within the detector. Although the determination of either the external or the internal gamma-ray spectrum is subject to multiple scattering corrections, the nature of these corrections is quite different in each case. The influence of multiple scattering can be eliminated during the unfolding of the internal electron recoil spectrum to obtain the external gamma-ray spectrum. To obtain the internal gamma-ray spectrum, however, corrections must be introduced to "resolve" the multiple scattered recoil electrons that are a component of the internal Compton recoil spectrum. It is the internal gamma-ray spectrum and the corresponding induced Compton recoil spectrum which are of substantial interest in radiobiology.

The most attractive advantage of scintillators for use in gamma-ray spectroscopy is the high event rate possible with larger detectors. A 12.7 cm diameter sphere of liquid scintillator gathers data about 300 times faster than the silicon detector described above. Consequently, any disadvantages arising from the required multiple scattering correction and the broader resolution of the scintillator are counterbalanced by the great improvement in statistical precision of the observed Compton electron continuum.

## Absolute Dosimetry

In materials of low atomic number, the gamma-ray spectrum induces a unique Compton recoil continuum which provides, in turn, the direct mechanism for gamma-ray energy deposition. The infinite medium dose rate  $\dot{D}_{IM}$  or, equivalently, the energy deposited per unit time and per unit mass by  $W_{IM}(E)$  in a material of low atomic number with electron density  $\rho$  (electrons per unit mass) is

$$\dot{D}_{IM} = \rho \int E \cdot W_{IM}(E) dE \quad (2)$$

Although equation (2) is only valid for infinite medium spectra, an immediate generalization is possible. The general relation between dose rate  $\dot{D}$ , and the recoil electron spectrum  $W(E)$  can analogously be written as

$$\dot{D} = \rho \int E \cdot W(E) dE \quad (3)$$

where  $W(E)$  is the actual recoil spectrum—measured in a silicon detector or a plastic or liquid scintillator—which can therefore include both finite size and multiple scattering effects. It follows that the dose rates based on equation (3) will automatically account for these effects. The infinite medium dose rate,  $\dot{D}_{IM}$ , of equation (2) represents a point dose rate value and so corresponds to an absorbed dose rate without perturbation of the gamma-ray spectrum. On the other hand, the

dose rate  $\dot{D}$  obtained from equation (3) properly accounts for the effects of the absorber on the external spectrum.

The fundamental advantage of the new approach must be stressed. By contrast with other methods it is clear that dose measurements based on equation (2) or equation (3) automatically account for the continuous energy distribution of the gamma-ray field. The absolute nature of these measurements depends intimately on the energy calibration of the electron spectrum. The correlation between detector response and electron energy may be accomplished in several ways but does not require a gamma source of known intensity. A mono-energetic gamma source is, for example, sufficient to determine the correlation.

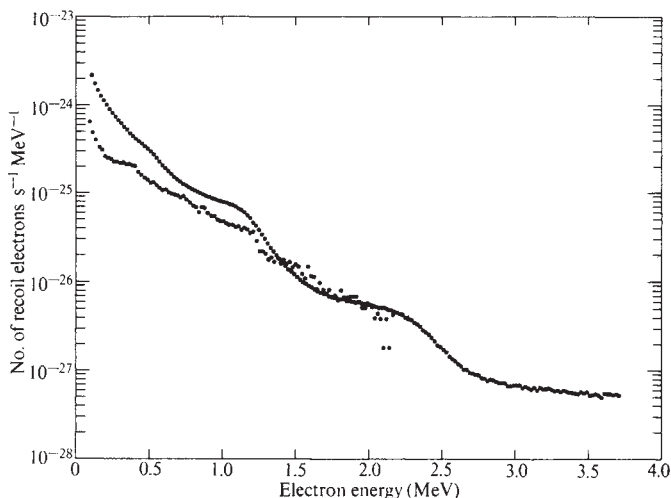


Fig. 1 Environmental electron recoil spectra observed at the Argonne National Laboratory site with a silicon solid state detector (lower curve) and a large liquid scintillator (upper curve). Each spectrum represents number of Compton recoil electrons per second per MeV and per detector electron.

Both the lithium-drifted silicon detector and the scintillators possess unique advantages for dose measurements. The silicon detector can be used for point dose measurements, but, on the other hand, the scintillators are inexpensive and can be formed to study average dose in specialized configurations. And by varying the proportional size of the scintillator, surface-to-volume effects can be studied. The two classes of detectors can be used together to investigate multiple interactions and the fundamental distribution of dose. The high sensitivity of the large scintillators makes them quite attractive for environmental measurements and particularly for the development of isodose curves.

## Measured Spectra

The induced Compton recoil spectra of two complementary detectors, a 12.7 cm diameter sphere of NE-213 (Nuclear Enterprises, Inc.) liquid scintillator ( $\sim 10^3$  cm<sup>3</sup>) and a lithium-drifted solid state silicon detector ( $\sim 1$  cm<sup>3</sup>), are shown in Fig. 1. Both spectra represent environmental measurements made at the Argonne National Laboratory site. The recoil electron spectrum in silicon was recorded in March 1970 and the spectrum in the scintillator was recorded in April 1971. In each spectrum, a Compton edge at 1.245 MeV due to the 1.461 MeV gamma emission of potassium-40 is apparent. The scintillator spectrum also displays a Compton edge at 2.38 MeV due to the 2.615 MeV gamma emission in the thorium series. The structure of the scintillator spectrum clearly lacks the resolution of the lithium-drifted silicon electron spectrum. But the broader resolution of the scintillator



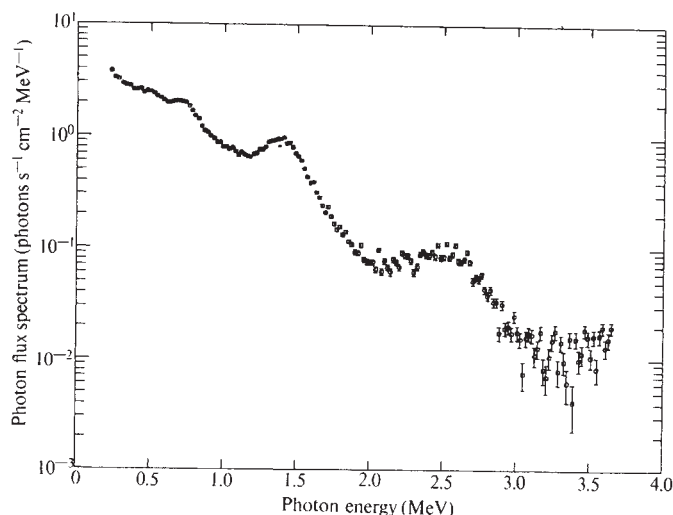


Fig. 2 Environmental gamma-ray spectrum measured by Compton recoil electron spectroscopy with a 12.7 cm diameter liquid scintillator at the air-land interface.

is partially compensated by the improved statistical accuracy available because of its greater size.

The energy scale of the silicon spectrum presented in Fig. 1 has been established with a caesium-137 gamma source. In silicon, there is sufficient photoelectric response to detect a photopeak at 0.661 MeV. No photopeaks due to mono-energetic gamma-ray sources are apparent in the response spectrum of the liquid scintillator because of the reduced photoelectric effect and the broader resolution of the scintillator; the energies of the Compton edges from laboratory sources of zinc-65, sodium-22 and tellurium-208 were therefore used. The calibration procedure involves the convolution of a calculated Compton electron distribution with a Gaussian broadening factor. Energy and broadening parameters are adjusted to

obtain the best least-squares fit between the measured electron recoil spectrum and the convoluted distribution. An agreement of 4.0% was obtained among the energy calibrations determined with these three different gamma-ray sources.

Fig. 2 represents a continuous gamma-ray spectrum unfolded by the direct application of equation (1) to the scintillator electron recoil spectrum of Fig. 1. The 1.461 MeV gamma emission of potassium-40 and the 2.615 MeV gamma emission in the thorium series are properly located in photon-energy space and this implies that proper calibration has been obtained in electron energy space. Even though no corrections were applied for multiple scattering, the spectral results shown in Fig. 2 are in qualitative agreement with environmental spectra determined with the silicon detector<sup>3,4</sup>.

Environmental dose rates have been obtained from the electron spectra in Fig. 1. A point dose rate of  $4.3 \pm 0.9 \mu\text{R h}^{-1}$  was observed with the silicon detector in the gamma-ray energy region 0.2 MeV to 2.2 MeV whereas a gamma dose rate of  $3.0 \pm 0.3 \mu\text{R h}^{-1}$  was observed with the scintillator in the gamma-ray energy region 0.2 MeV to 3.0 MeV. The dose rate observed with the scintillator is almost unchanged if the data are restructured to correspond to the gamma-ray energy range of the silicon detector. Although the two dose rates are barely outside experimental error, no agreement is implied because the two experiments were carried out at times separated by many months. It is anyway expected that the dose rate results would differ for these two detectors because the size of the scintillator can produce a perturbation of the environmental gamma-ray field.

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# Selective Volatilization on the Lunar Surface: Evidence from Apollo 14 Feldspar-Phyric Basalts

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**Analysis of Apollo 14 samples has revealed a basalt that could be parental to the highlands anorthosites.**

ONE of the most strongly contested arguments among scientists investigating the chemistry of lunar rocks is the cause of their low content of volatile elements. In particular, the mare basaltic

lavas are notably depleted in sodium and potassium compared with otherwise equivalent terrestrial basalts<sup>1,2</sup>. The three hypotheses at present are (1) the Moon was volatile-deficient at the accretionary stage, due either to higher peripheral temperatures or to a lower accretion efficiency than the Earth<sup>3,4</sup>; (2) certain elements, including K and Rb, became concentrated at an early stage in a sulphur-bearing core<sup>5</sup>; (3) volatiles were lost from the lavas during eruption and flow in the low-pressure environment of the lunar surface<sup>6,7</sup>.

A second important problem concerns the nature and origin of the Moon's crust. If, as seems likely, this is an anorthosite<sup>8</sup>

there are serious problems in finding a source for this material among the mare basalts. To obtain such a monomineralic rock (melting temperature 1,530° C) requires feldspar accumulation from a lower temperature (1,100 to 1,200° C) basalt magma. The Apollo 11 and 12 basalts failed, however, to show textural or experimentally derived evidence for a feldspar phase on the liquidus. Associated with this problem is the europium anomaly in the basalts, which suggests that plagioclase feldspar (carrying  $\text{Eu}^{2+}$ ) did, in fact, separate from more primitive basalts with normal rare-earth abundance patterns. Thus the question that arises is whether the primitive lunar crust was derived from a more aluminous basalt than that collected from the mare regions and, therefore,

much richer<sup>9</sup> in K and Rb than the "high K" basalts of Apollo 11. It is safe to assume that with such a composition and with about 10% of large phenocrysts, plagioclase crystallized on or very close to the liquidus. Such a basalt could produce anorthosite by floating of the phenocrysts but, if so, it should not, in the undifferentiated state, show the full europium anomaly characteristic of the plagioclase-depleted mare basalts.

**Table 1** Plagioclase Feldspar Compositions by Electron Probe Analysis

| Specimen | Crystal generation* | Ca/(Ca+Na+K) atomic % | Na <sub>2</sub> O wt % | K <sub>2</sub> O wt % |
|----------|---------------------|-----------------------|------------------------|-----------------------|
| 14310/13 | PC                  | 91.5                  | 0.91                   | 0.09                  |
|          | PI                  | 85.5                  | 1.52                   | 0.25                  |
|          | PR                  | 75.4                  | 2.42                   | 0.52                  |
|          | PC                  | 92.5                  | 0.77                   | 0.12                  |
|          | PI                  | 90.0                  | 1.04                   | 0.13                  |
|          | PR                  | 87.0                  | 1.29                   | 0.25                  |
|          | PC                  | 95.0                  | 0.51                   | 0.08                  |
|          | PI                  | 93.5                  | 0.64                   | 0.15                  |
|          | PR                  | 86.0                  | 1.42                   | 0.27                  |
|          | GL                  | 94.5                  | 0.51                   | 0.15                  |
|          | GML                 | 91.0                  | 0.77                   | 0.35                  |
|          | GML                 | 92.5                  | 0.77                   | 0.15                  |
| 14310/20 | P                   | 90.5                  | 1.00                   | 0.14                  |
|          | P                   | 90.0                  | 1.06                   | 0.12                  |
|          | P                   | 91.5                  | 0.86                   | 0.14                  |
|          | P                   | 91.5                  | 0.86                   | 0.12                  |
|          | GL                  | 93.0                  | 0.73                   | 0.14                  |
|          | GL                  | 93.0                  | 0.73                   | 0.09                  |
| 14073/9  | PI                  | 91.5                  | 0.86                   | 0.13                  |
|          | PI                  | 88.0                  | 1.21                   | 0.21                  |
|          | PR                  | 72.5                  | 2.67                   | 0.59                  |
|          | PC                  | 93.0                  | 0.73                   | 0.13                  |
|          | PI                  | 85.0                  | 1.45                   | 0.39                  |
|          | PR                  | 67.0                  | 3.14                   | 0.73                  |
|          | PC                  | 91.5                  | 0.86                   | 0.13                  |
|          | PI                  | 87.0                  | 1.34                   | 0.19                  |
|          | PR                  | 86.5                  | 1.34                   | 0.21                  |
|          | GL                  | 94.0                  | 0.61                   | 0.13                  |
|          | GML                 | 91.5                  | 0.86                   | 0.15                  |
|          | GML                 | 94.0                  | 0.61                   | 0.09                  |

\* P, Apparently unzoned phenocryst; PC, zoned phenocryst core; PI, zoned phenocryst intermediate sheath; PR, zoned phenocryst rim; GL, groundmass lath (unzoned); GML, groundmass micro-lath in residual mesostasis.

Analyst: A. Peckett.



**Fig. 1** Photomicrograph of feldspar-phyric lunar basalt (14310/20). The separate generations of rectangular plagioclase feldspar phenocrysts (colourless) and criss-cross laths of groundmass plagioclase are clearly visible. The laths are intergrown chiefly with Ca-poor pyroxene.

whether the accretion of the Moon produced a layered structure from which basalts of differing composition could be generated at differing times and depths. A third problem is whether the sampled lavas are derived directly from the lunar mantle or whether they have suffered crystal differentiation near the lunar surface.

## Fra Mauro Specimens

We have been studying basalt specimens from the recently allocated suite of Apollo 14 specimens, collected from the Fra Mauro formation. Unfortunately, apart from a preliminary report<sup>9</sup>, little information has yet been published on this suite. We cannot, therefore, carry our conclusions as far as we would wish in the absence of refined data on whole-rock compositions, europium contents and the model and isochron ages of the rocks we have examined. Even so, we have obtained information which goes some way to answering the problems mentioned previously and which raises new questions on several lunar geochemical and evolutionary topics.

First, the two basalts received from Houston (14310 and 14073) are each clearly feldspar-phyric (for example, Fig. 1) and are the first lunar basalts to show this property. Also, they are much richer in total plagioclase feldspar (average 53%) than the Apollo 11 and 12 basalts (25 to 40%), as evidenced by the  $\text{Al}_2\text{O}_3$  content of 14310 (20% by weight<sup>9</sup>). This rock is also

Second, we have analysed thirty crystals of plagioclase (Table 1) by electron probe analysis (performed by A. P.) in three polished rock sections—14310/13, 14310/20 and 14073/9. There are several generations, with contrasted compositions. Briefly, the phenocrysts are strongly zoned to more sodic and potassic compositions whereas the abundant groundmass laths (Fig. 1), and even the micro-laths in the residual mesostasis, are as low in sodium and potassium as the phenocryst cores. In view of these data, we propose that the basalt lavas lost Na and K on the lunar surface. Also, we propose that the lavas were erupted in two stages, the first of which would permit appreciable, near-surface crystallization differentiation of once-primitive lavas. As far as selective volatilization is



concerned, we must question whether analyses of surface-erupted lunar basalts can be used to postulate a unique, alkali-poor composition for the whole Moon.

Specimen 14310 is from a 3.4 kg basalt block, and 14073 is a soil fragment from the base of a 60 cm deep trench (both Station G)<sup>9</sup>.

## Analytical Data

Modal point-counts are not particularly accurate, so we can only propose a plagioclase content, for 14310/20 (Fig. 1), of  $53 \pm 3\%$ ; the possible error does not influence the subsequent argument. Even more difficult is the calculation of the phenocryst core plus rim contents, which we estimate to be 10%. The remaining 43% of plagioclase is present as groundmass laths, producing the dominating ophitic texture with Ca-poor pyroxene (Figs. 1, 2) and as small patches of micro-laths in a residual mesostasis (Fig. 3). The last of these includes  $\leq 1\%$  of analysed barian sanidine and potassic rhyolite glass which are used in the K-content calculations (Table 2). Otherwise the rock contains relatively small amounts of the phases that we have found in other Apollo basalts (nickeliferous iron, troilite, ulvöspinel, ilmenite, pyroxferroite, whitlockite, baddeleyite) but no olivine or cristobalite and very little calcic pyroxene and chrome-spinel.



**Fig. 2** Photomicrograph showing normal zoning of large plagioclase phenocryst, which has a sodic rim (14310/13). Groundmass of calcic plagioclase laths and pyroxene, the laths being discontinuous with the phenocryst rim. Crossed polars.

The plagioclase analyses are reduced simply to "anorthite" values (atomic  $\text{Ca}/(\text{Ca} + \text{Na} + \text{K})$ ) and the weight percentages of  $\text{Na}_2\text{O}$  and  $\text{K}_2\text{O}$  (Table 1). Because of possible criticisms of generalized data, we have given the data for all analysed grains.

There are no notable exceptions to the following conclusions. The phenocrysts have large, homogeneous cores as calcic as Ca95, with a mean value of Ca94. Most phenocrysts have a discontinuity between core and rim, the rims being zoned continuously (Fig. 2) as low as Ca67 in one instance. (It is not possible to detect and measure extreme compositional trends on all the outermost rims.) We can safely assume, however, that the magma was only capable of precipitating material of a less calcic composition than Ca67 at the phenocryst rim stage. In

marked contrast, the abundant, groundmass plagioclase crystals (Fig. 3) are much more calcic than the phenocryst rims. The measured range is Ca94.5 to Ca91 (the latter being potassic rather than sodic), for which we take a grain-number mean of Ca93. The values are given as Ca% for convenience, but the significance lies in the associated pattern of distribution of  $\text{Na}_2\text{O}$  and  $\text{K}_2\text{O}$  (Table 1). In cases where the amounts of these two oxides are small, experimental error reduces the significance of minor differences. We note, but do not use in the calculations, a mean value of 0.61%  $\text{Na}_2\text{O}$  and 0.12%  $\text{K}_2\text{O}$  for the phenocryst cores (Ca94). For the phenocryst rims, we must take the most sodi-potassic example analysed (Ca67) because this represents the minimum range of zoning. For this rim, the values are 3.14%  $\text{Na}_2\text{O}$  and 0.73%  $\text{K}_2\text{O}$ . Finally, the calcic groundmass laths average at Ca93, with 0.70%  $\text{Na}_2\text{O}$  and 0.13%  $\text{K}_2\text{O}$ . There is no significant difference between these and the phenocryst cores. Also, the same pattern of reversal would arise from the selection of any triplet (core, rim, lath) from Table 1.



**Fig. 3** Photomicrograph showing three generations of plagioclase feldspar (14310/13). A small, normally zoned phenocryst (centre); abundant, calcic groundmass laths; and a residuum area with calcic micro-laths (top centre). Crossed polars.

## Alkali Loss

The normal zoning of the calcic plagioclase phenocrysts to Ca67 leads to the assumption that, under normal magmatic conditions, the groundmass plagioclase would be at least of the same composition. Certainly it could not, according to phase relations in the system An-Ab-Or, be more calcic and poorer in Na and K. As we have seen, however, the Apollo 14 basalts show abundant groundmass plagioclase crystals with this property. In place of the normal cooling process of a closed system, the only mechanism likely to produce such a drastic effect would be the loss of Na and K from the system at this stage. Non-equilibrium, rapid cooling of a closed-system magma would still have produced a less calcic plagioclase than that of the phenocryst rims.

A very conservative estimate of the alkali loss is shown in Table 2. The unknown factor is the "potential plagioclase" in the magma before loss of volatiles, and we have therefore retained a known, minimum value in terms of  $\text{Na}_2\text{O}$  and  $\text{K}_2\text{O}$  content. We can estimate with some certainty, however, the normative plagioclase in a liquid that had just precipitated the Ca67 (or An67) rims to the phenocrysts. From the simple synthetic system An-Ab, the normative liquid plagioclase would be An26. More closely, from natural polycomponent

systems, the liquids from which plagioclases have crystallized in equilibrium have been calculated in a layered intrusion<sup>10</sup>. These show An<sub>67</sub> in equilibrium with An<sub>56</sub> liquid. The "liquid" analysis<sup>10</sup>, recalculated in terms of its plagioclase content, shows about 5% Na<sub>2</sub>O, and most published analyses of An<sub>56</sub> crystals indicate about 8% Na<sub>2</sub>O. The K<sub>2</sub>O values are too low to be significantly altered by this refinement. The value of 3.14% Na<sub>2</sub>O in Table 2 could legitimately be increased by a factor of 2, and the value for K<sub>2</sub>O could probably be increased by a factor of about 1.5, strengthening the argument concerning alkali loss even further. This potential plagioclase in the liquid would, of course, crystallize as more than one phase (sodic plagioclase, potassic feldspar, and possibly a syenitic residual glass) as the temperature dropped.

**Table 2** Calculation of Alkali Loss during Volatilization

|                      | Phase                                    | Vol. % in magma/rock* | Na <sub>2</sub> O (wt %) | K <sub>2</sub> O (wt %) |
|----------------------|------------------------------------------|-----------------------|--------------------------|-------------------------|
| Before volatile loss | Potential plagioclase, Ca <sub>67</sub>  | 44%                   | 3.14                     | 0.73                    |
| After volatile loss  | Groundmass plagioclase, Ca <sub>93</sub> | 43%                   | 0.70                     | 0.13                    |
|                      | K-feldspar (K <sub>2</sub> O=13%)        | 0.5% max.             | 0.00(5)                  | 0.06(5)                 |
|                      | K-rhyolite (K <sub>2</sub> O=7%)         | 0.5% max.             | 0.00(4)                  | 0.03(5)                 |
|                      | Total of three phases                    |                       | 0.80                     | 0.23                    |
| Volatile weight loss |                                          |                       | 2.34                     | 0.50                    |
| % loss from original |                                          |                       | 74                       | 68                      |

\* Modal volume used only for calculating relative contribution of the three phases analysed in the rock. The "potential plagioclase" is equated with the extreme, analysed phenocryst rim (Table 1) and would be an under-estimate of original, potential Na<sub>2</sub>O and K<sub>2</sub>O. Percentage losses are therefore likely to have been even greater than calculated.

The minimum values calculated in Table 2 show that the loss of Na and K has been appreciable, amounting to 74% Na<sub>2</sub>O and 68% K<sub>2</sub>O loss. Using the refinement outlined above, these values would be 86% Na<sub>2</sub>O and 78% K<sub>2</sub>O.

An experimental study of selective volatilization has been carried out by G. M. Biggar (personal communication). He used a terrestrial basaltic rock (Scourie dyke, iron-rich tholeiite) as the starting material, at  $f_{O_2}=10^{-12}$  bar and with original contents of 2.38% Na<sub>2</sub>O, 0.92% K<sub>2</sub>O. There was appreciable loss of these oxides and, in extreme cases, depletion to 0.13% Na<sub>2</sub>O, 0.12% K<sub>2</sub>O occurred. Close comparison with our observations is possible for less extreme experiments which show 75% loss of Na<sub>2</sub>O and 73% loss of K<sub>2</sub>O (compare Table 2). These losses occurred during experimental runs of 24 to 60 h at 1,160 to 1,175° C and at pressures between  $1.5 \times 10^{-4}$  and  $2 \times 10^{-5}$  torr.

It follows that the conditions on the lunar surface, where basaltic magmas were erupted at about 1,100 to 1,200° C in an atmosphere with a pressure of the order of  $10^{-12}$  torr, would be appropriate for alkali loss during flow for 1 or 2 days.

## Pyroxenes

The plagioclase-phyric basalts contain abundant pyroxene (Fig. 1), most of which has crystallized ophitically with the small calcic plagioclase laths. Small cores to each pyroxene may, however, have crystallized before volatile loss. These cores are the lowest in Ca and highest in Mg of any lunar pyroxene so far analysed from a basalt. They are close to Ca<sub>3</sub> Mg<sub>81</sub> Fe<sub>16</sub> and zone outwards to more iron-rich pigeonites, and eventually through subcalcic augite to pyroxferroite. Hence the Apollo 14

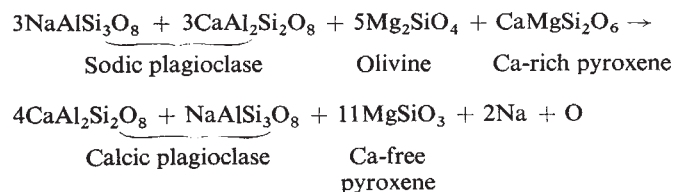
pyroxenes are more primitive pyroxenes than those found in Apollo 12 basalts, which in turn were more primitive (pigeonite-rich) than those in the Apollo 11 basalts. The more magnesian character of the Apollo 14 pyroxenes is more closely comparable with those in the norite fragments<sup>11</sup> from the regolith at all three sites than with the mare basalts. Perhaps the specimens under discussion should be called norites; if so, they provide better textural evidence for a magma of this type than has so far been obtained from the frequently shocked regolith fragments<sup>12</sup>. They also show, in their late pyroxene trends and in the character and texture of the KREEP residual material, close analogy with the crystallization trends in the Apollo 12 mare basalts<sup>11</sup>. The pyroxenes are less aluminous than in the mare basalts, probably because of co-precipitation with abundant early plagioclase. The Al:Ti ratios are very high (up to 7:1), despite their low-pressure crystallization, indicating a low Ti activity for these aluminous basalt liquids. Soil fragments at the Apollo 14 site have been found to contain similarly magnesian orthopyroxenes to those in the feldspar-phyric basalts (Ca<sub>3</sub> Mg<sub>81</sub> Fe<sub>16</sub>). Some also contain olivine of Mg<sub>90</sub> Fe<sub>10</sub>, indicating a minimum lunar mantle composition of Mg/(Mg + Fe) = 0.9.

## Two-stage Process

The lunar basalts are either derived directly from the lunar mantle as primary liquids (A. E. Ringwood and co-workers), or they are secondary magmas that have been differentiated by low pressure crystal fractionation acting on the primary liquids (M. J. O'Hara and co-workers). If the latter is true, the lavas cannot be used to calculate the composition of the lunar mantle with which they would have been in equilibrium at the stage of primary partial-melt formation.

Our evidence suggests a two-stage magma eruptive process. If the specimens represent solidified lava-lake material alone, the plagioclase phenocryst zoning should have continued beyond Ca<sub>67</sub>, into the groundmass. If they represent solidified surface lava, however, the plagioclases should all be alkali-depleted and of the same composition as the groundmass (Ca<sub>93</sub>). The evidence is that two environments were involved, the first permitting closed-system cooling to give zoned phenocrysts, and the second producing open-system alkali loss. Such evidence was lacking in the Apollo 11 and 12 basalts, where only "groundmass" plagioclases are involved; they are generally very calcic, although slight zoning may indicate some crystallization from thick lava flows. We propose a model to account for these Apollo 14 basalts, which also has implications for the genesis of anorthosites and of other crystal cumulates.

Mantle-derived basalt magma was poured into large, meteorite-scoured surface basins to form large-volume lava lakes. In the lake under consideration, the magma was probably a high-alumina tholeiitic basalt. Plagioclase crystallized early, probably accompanied by aluminous spinel and low-aluminous orthopyroxene. Some alkalis were probably being lost even at this stage (close to the lunar surface) to give very calcic plagioclase phenocrysts (Ca<sub>95</sub>). Loss of Na could also have inhibited olivine and Ca-rich pyroxene crystallization<sup>13</sup>, in favour of Ca-poor pyroxene:



The calcic plagioclase phenocrysts floated to the top of the lava lake to form a thick, shielding crust. They could carry with them a lot of small, attached, magnesian orthopyroxene crystals. The density difference<sup>12</sup> between the plagioclase (2.7) and the



liquid (3.0 to 3.2) would, in fact, permit floating of composite grains in the ratio of plagioclase : orthopyroxene (3.3) of about 1:1. This is often overlooked in explanations of magnesian minerals in anorthosites, but it is a common phenomenon in layered intrusions<sup>10</sup>. During this crystal-sorting process, denser minerals would settle to the floor. It is, therefore, less likely that we shall find impact disruption at this level to give many mafic cumulates on the lunar surface. Liquids could, however, be tapped with varying amounts of suspended, accumulated mafic crystals from below the plagioclase-enriched layer. As solidified lava with variolitic groundmass, similar to some of the Apollo 12 rocks (for example, 12040), they would tend to show a mafic phase on the liquidus in experimental studies. This would obscure the fact that plagioclase was a liquidus phase in the lava lake. In other words, crystal sorting in a near-cotectic liquid would yield products with diverse "liquidus" phases. It would mean that anorthosites could be derived from the same liquid as a "mare basalt", the anorthosites being enriched and the basalts depleted in europium.

At the top of the primitive lava lake, heat losses would be rapid and a temperature gradient would develop between roof and floor. Convection currents would then transfer heat from the plagioclase-rich roof zone, thus permitting adcumulus growth<sup>10</sup> to yield anorthosite with subsidiary magnesian mafics having little or no compositional zoning. After a thick anorthosite crust had formed, convection would diminish. The floated plagioclases would be less abundant and suspended in magma which, on cooling, would produce zoning of the plagioclase cores. At this stage, disruption of the crust by impact events probably occurred in certain lava lakes. The crust was pulverized to produce anorthosite and pyroxenic anorthosite fragments which were severely shocked and distributed widely across the lunar regolith. The underlying lava was then erupted to give thin flows of feldspar-phyric basalt of the type described here. The zoning stopped because alkali loss occurred on a large scale, and the lava precipitated calcic alkali-poor plagioclase.

If this model is applicable, we are now looking at volcanic rocks from the lunar surface that have been erupted after a period of crystal sorting. They are not in any sense primary magmas, although the bulk of anorthosite may ultimately point to a high-aluminium basalt as a dominant primary liquid. The detailed evidence provided by the Apollo 14 feldspar-phyric basalts can thus help to piece together several isolated problems, such as the genesis of anorthosites and the question of secondary magmas and of alkali volatilization. Restraints would then be imposed on models relating to the nature of the lunar mantle and the original lunar chemistry in terms of volatile elements. We do not wish to imply that there must be a direct historical connexion between the feldspar-phyric basalt—norite—anorthosite suite and the mare basalts. There is, however, a feasible connexion in terms of magmatic processes. Slight differences in primary magma composition, and differing timescales for the breaching of lava lakes and the loss of volatiles, would lead to appreciable differences in crystallization sequences and phase equilibria.

Fractionation of plagioclase in the lunar crust probably occurred at 4.6 b.y. on a much greater scale than in lava lakes. We are more concerned with the fact that the same type of primary basalt could have been present in later lava lakes, illustrating more clearly the processes associated with anorthosite—norite—feldspar-phyric basalt cogenesis. Plagioclase removal would be early, and would therefore not introduce significant Rb-Sr fractionations<sup>14</sup> during subsequent crystal sorting to produce the type of diversity seen in the Apollo 12 basalts. We favour crystal sorting, discussed above, for the anorthosite—norite suite, predominant as the "magic component" in the lunar regolith soils. Such a process would produce variable adcumulus effects, and hence variation in the Fe/Mg ratio of the silicates to give Type A and Type B anorthosites<sup>12</sup>, without a compensating variation (by either zoning or reaction) in the alkali-depleted plagioclases formed near the lunar surface. Fractionation of lunar basalts gives rise to phases rich

in K, REE, P and Zr. These are associated with rhyolitic glass which probably had a high viscosity in the liquid state and was immiscible with the ferrobasalt liquid<sup>15</sup>. This KREEP-rich liquid appears to have concentrated among the uppermost plagioclase-pyroxene, noritic cumulates (but not among the extreme adcumulus anorthosites). It is also present in isolated patches in the mare basalts and in the feldspar-phyric basalts. We consider it to be an *in situ* residual fractionate<sup>11</sup>, but it has a high Rb/Sr ratio and may be a contaminant<sup>14</sup> from a rhyolitic fraction produced earlier. It would melt easily and could be the contaminant necessary to explain the variable initial <sup>87</sup>Sr/<sup>86</sup>Sr in the mare basalts<sup>14</sup>. Again, the idea that a single lava lake could produce various erupted basalt lavas is still a possibility, although a multi-lake terrain would superimpose greater variety.

We would like to see europium analyses made on the Apollo 14 feldspar-phyric basalts, because the plagioclase phenocrysts ought to provide evidence for or against the hypothesis<sup>16-18</sup> that Eu<sup>2+</sup> enters preferentially into the plagioclase structure. Age data on these rocks would also be highly significant; if they are associated with an early, crustal anorthosite generation, they ought to yield isochron ages of the order of 4.6 b.y.; but if they yield younger, mare-basalt type ages (with old model ages) then we must either accept a metamorphic event of that age (difficult, in view of preserved feldspar zoning and residual glass) or consider closer genetic relationships between the feldspar-phyric, high alumina basalts and the low-alumina mare basalts.

We thank Professor M. J. O'Hara for many constructive comments, for providing the unpublished experimental data from volatilization experiments and for the loan of specimen 14310/13. We also thank our co-investigators at Durham, Dr C. H. Emeleus, Dr J. G. Holland and Mr R. Phillips, for related lunar-rock data. The research is supported by the Natural Environment Research Council.

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# LETTERS TO NATURE

## PHYSICAL SCIENCES

### Model of Early Lunar Differentiation

It has been suggested that one of the first major chemical differentiations in a planetary body such as the Earth will involve segregation of an Fe-FeS liquid<sup>1,2</sup>. This is because the eutectic temperature of this system (referred to here as Fe-S) is 990° C and is much lower than the temperature required to melt silicates (~1,100° C). The eutectic temperature is relatively insensitive to pressures<sup>3</sup> of at least 30 kbar.

The surface regions of the Moon have been melted and silicates differentiated at ~4.6 billion yr ago<sup>4,5</sup>. It is certain, that any Fe-S present will melt long before the silicates and because of its greater density will sink into the interior. Because of the small gravitational size of the Moon and because of the time required to accumulate any radiogenic heat, the interior of the Moon at the time of surface melting must have been cold; thus the sinking Fe-S liquid will be trapped at some depth.

Sonnett *et al.*<sup>6</sup> have described evidence for a layer of high electrical conductivity in the Moon at a depth of 250 km. They suggest that this is caused by compositional or phase change. It is improbable that phase changes, caused by partial melting in the silicate compositions appropriate to the Moon, can result in an electrical conductivity increase of three orders of magnitude. More probably, a major compositional change is involved. We suggest that this is an Fe-S layer (Fig. 1). In a Moon containing, as in some meteorites, about 5% by weight of FeS, melting of the outer 200 km will produce an Fe-S layer roughly 10 km thick.

If the Moon was an Earth satellite in synchronous rotation at the time<sup>7</sup> of melting, tidal forces and the then rapid rotation of the Moon would produce bulges in the Fe-S liquid layer both towards and away from the Earth with associated thinning at the poles. Further, differential gravitational effects between the rigid interior and the surrounding Fe-S liquid layer could introduce a basic asymmetry in the mass distribution of the Fe-S layer, the mass being greatest in the Earth-facing side

(Fig. 2), analogous to models suggested by Baldwin<sup>8</sup> and Smith *et al.*<sup>9</sup>. Such a distribution of mass in the Fe-S layer is in qualitative agreement with the three unequal lunar moments of inertia<sup>10</sup>. Hydrodynamic instabilities of the type described by Elsasser<sup>11</sup> can produce downward bulges in the layer and consequent localized concentrations of Fe-S. We shall refer to these Fe-S concentrations as "fescons". We expect the fescons to be preferentially located on the near side of the Moon, reflecting the asymmetry of mass in the Fe-S layer caused by the tidal forces.

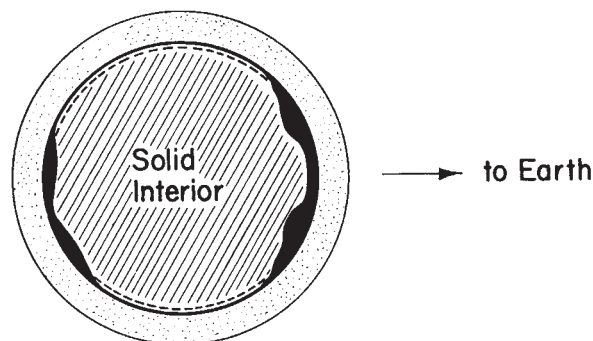


Fig. 2 "Droplets" of Fe-S form fescons at sites of tidal bulges.

The Moon's observed principal moments of inertia cannot result from a homogeneous body having the Moon's known shape. Muller and Sjogren<sup>12</sup> have shown that the circular maria are associated with positive gravity anomalies attributed to excess mass (mascons) under these regions. Mascons near the surface<sup>13-15</sup> can explain quite precisely the known ratios between the lunar moments of inertia. But their masses are a factor of ten too small to account for the magnitude of the moments of inertia<sup>16</sup>. In our model, variations in the thickness of the deformed Fe-S layer from 10-20 km produce fescons of sufficient mass ( $10^{21}$ - $10^{22}$  g) to explain the observed moments of inertia.

Using the treatment of Morgan<sup>17</sup> and assuming a viscosity in the lunar interior of  $10^{26}$  poises<sup>18</sup>, a fescon of mass  $10^{22}$  g would sink only 40 km in 4.5 billion yr, even if the mass were in the form of a uniform sphere. Because the fescons would probably be disk-shaped, the excess pressure, and hence the rate of sinking, would be appreciably less. Any minor sinking would tend to produce subsidence of the overlying material resulting in shallow circular basins.

It has been argued recently that in conditions of Fe-S segregation, the normally lithophile alkali metals, K, Rb and Cs, may exhibit chalcophilic behaviour<sup>19,20</sup>. In the Moon, therefore, we might expect that the segregation of Fe-S from the surface materials has impoverished them not only in chalcophile and siderophile elements, but in the alkalis as well. Heat production in the fescons by decay of <sup>40</sup>K with a half life of 1.3 billion yr would be most pronounced in the first billion years or so of lunar history, while the U and Th heat production in the outer zone of silicates would be both more uniform with

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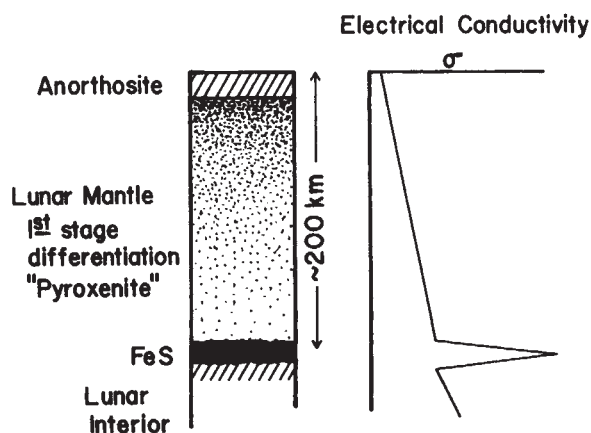


Fig. 1 Position of Fe-S layer in the Moon  $4.5-4.6 \times 10^9$  yr ago.



## REVIEW SUPPLEMENT

# Slowly but Steadily with Information

THE doctrine of the free flow of scientific information is a little like the concept of democracy—everybody is for it in theory but most people hope against their better judgment that they will not themselves be too seriously diverted from their individual objectives. At the beginning of October, UNESCO held a conference in Paris to make plans for the development of what is called a "World Science Information System". The occasion, no doubt the first of many, was organized around a careful study report prepared in the past three years by a committee of sensible men sponsored jointly by the International Council of Scientific Unions and UNESCO. Naturally enough, those who worked hard, sometimes with stars in their eyes, to make the conference a success will have been disappointed that it turns out to be less concerned with the proper organization of the exchange of information than with the question of whether the proposals put forward in Paris two months ago would give a fair crack of the whip to developing countries. The complaint seems to have been that only two of the twenty-two recommendations put forward for consideration by the conference dealt specifically with the problems and needs of developing countries. "Is this not a measure of how we are neglected?" was the cry. The simple answer, and the truth, is of course that in developing machinery for the exchange of scientific information, it is in everybody's interests that the methods now developed should serve not merely for the present and the immediate future but for the decades ahead when, with any luck, many of the developing countries will have reached and even surpassed the stage of development which pertains in societies now considered to be advanced. The developing countries are right constantly to remind their well-to-do competitors that foreign aid and technical assistance are at present being provided on too niggardly a scale, but that is quite a separate matter.

The proposals which form the bread-and-butter of the conference in Paris are known by what seems to be a code name—UNISIST. In modern language, to describe the plans now made as a "World Science Information System" is over-ambitious and even misleading. What the working group responsible for the documents discussed in Paris seems to have had in mind is that it might be possible, by timely and quick-witted intervention from an international base, to influence the development of national policies on the handling of scientific information in such a way as to bring about valuable economies, both in direct cost and in the saving of scientific effort that might otherwise be wasted on duplicated research. This is a sensible approach to a problem or set of problems for which there can of course be no ideal solution—the needs of those who use scientific information and the character of scientific information itself are changing too quickly for it to be feasible to design in advance an information exchange system that will last for the rest of time. The difficulty is, of course, that in attempting to

coordinate national activities in the interests of efficiency and economy, success requires not merely an acknowledgment that coordination would be worthwhile but also a degree of flair. It remains to be seen whether UNESCO will be able to bring to this task more than the pedantic bumbledom with which it usually tackles jobs like this.

For the time being, it seems that there will be no separate budget for work under the UNISIST banner, but that it will be possible to scrape together from the existing UNESCO budget enough money to begin work on a new World List of Scientific Periodicals and to prepare for a more ambitious programme in 1973. This perhaps is just as well. No harm will be done if those concerned with the processing of scientific information have a little more time to think about the practical implications of the recommendations discussed in Paris. And the first question to be asked, not merely by the professional processors of scientific information but also by those whose activities in research laboratories give rise to scientific information, is what the function of the free exchange of scientific and technical information should be.

This is a field in which means are too easily and too frequently confused with ends. The purpose of exchanging scientific information is now what it has been for the past three centuries—to stimulate the progress of research and to provide practical assistance to those who may be able to apply the results of scientific research. In basic research, especially that of an academic character, there is no doubt that everybody profits from a full exchange of information but, at the same time, it does appear that the problems of exchanging information as such are comparatively easily mastered. The people who work in universities are, after all, usually able to keep up with what their colleagues elsewhere are doing by correspondence, by using the scientific journals, by gossiping at conferences and by using their scientific colleagues (and particularly their research students) as news hounds of a kind. In this context, however, the most difficult task in the exchange of information is not so much the provision of references to published literature, lists of titles or indexes of key words as the almost pedagogical job of helping people to understand what their colleagues elsewhere are driving at. The schemes that have been worked out in the past few years for mechanizing the scientific literature or at least the process of indexing it are naturally of great value to academic scientists, but in the last resort there is no substitute for the hard work of reading and puzzling

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out what the other fellow means. In short, UNISIST (or some other coordinating body) could help with the flow of information within the academic part of the scientific community, but its influence would not be crucial, whatever may be said (as in the UNISIST report) of the "information explosion".

Formal arrangements for the exchange of information are likely to be of greater value in applied research and development. This, certainly, is at least a part of the reason why nuclear engineering has seen the most elaborate development of information processing and exchange systems, first on a national basis under the aegis of organizations such as the United Kingdom Atomic Energy Authority and the United States Atomic Energy Commission and then internationally, within Euratom, for example, and within the International Atomic Energy Agency in Vienna. These are eminently fields in which factual information arising from research can help directly with the design of equipment, systems or research programmes elsewhere. The difficulty, of course, is that there are bound in circumstances like these to be severe limits to the altruism of those with information to give away. It is a considerable triumph that there has been as close a degree of sharing of information on nuclear energy as the International Atomic Energy Agency has been able to arrange. Obviously there is scope for a similar treatment of some other fields—tropical agriculture and medical research are obvious examples—but it is bound to be a long time before those who develop electronic components made of microcircuits, or photographic emulsions for special purposes, will be willing to see full information disseminated internationally. In other words, those who set about the design of systems for the free exchange of information must recognize that the uses to which these systems will be put will be in part at least determined by the compliance of the sources of information as well as by the demands made on the systems by the users. A more hard-headed discussion of this feature of the process would have strengthened the study report which provided the agenda for the meeting in Paris.

Within such a framework, there is a great deal of useful work that can be done. The UNISIST group quite properly makes a good deal of the work that needs to be done in standardizing the methods at present used for describing scientific information. Even on matters such as standards of bibliographic description, there is immense chaos. Although several bodies, national and international, have in the past set out to promulgate standards that would be internationally recognized, the chief result of these well-intentioned efforts so far has been not standardization but diversity. There are countless illustrations of how those first in the field of standardization have found themselves lumbered with antiquated arrangements—the 7-bit computer code developed in the United States in 1963 for ensuring some degree of compatibility between computers turns out to be less satisfactory than and incompatible with the 8-bit code now favoured by the International Organization for Standardization. And then there are fierce problems to be solved in abstracting and indexing. If UNISIST chooses to cut its teeth on problems like these, it will have a decade of hard and useful work ahead of it and long before that time is up it will have worked out an indispensable place for itself in the international scientific community.

To the extent that the specific programme outlined in Paris (and not yet financed by UNESCO) is of this nature, nobody will complain. Unfortunately, there is a tendency in the document for ambition to run ahead of what may be feasible. The report asks, for example, that there should be an attempt at this stage to enlist the cooperation of the International Telecommunications Union and similar bodies in experiments on the mechanics of transferring scientific information in printed or telegraphic form from one place to another. There are schemes for using Earth satellites in the same connexion. The UNISIST programme also asks for programmes for the evaluation of efficiency in the handling of scientific information (which is sensible and necessary) and for the evaluation of research and information science (which on the face of things would be best left to the organizations which are responsible, on a national basis, for sponsoring research). The most obvious difficulty, of course, is that if UNISIST is to spread what little funds it can command over such a wide and difficult field of inquiry, it will find that it can do nothing properly and its reputation for good sense will be lost before it can be used. In all the circumstances, there is a great deal to be said for modesty and the containment of ambition. That is another reason why no harm may be done if UNISIST has to kick its heels, for another year or so, waiting for the money to come through.

UNISIST is on still more dangerous ground in what it has to say about the development of policies on the exchange of information. To be sure, the need that some information should be kept secret because governments want it kept secret is acknowledged, but grudgingly. The UNISIST document considers that there is even less reason for confidentiality in the sharing of information which may be of economic or industrial importance—the difficulty here is not so much that free exchange is unlikely to be possible but that by pretending that free exchange is ultimately inevitable, UNISIST runs the risk of seeming unrealistic. (Exactly the same criticism can be levelled at the UNISIST declaration on copyright, which the report describes as "another form of information monopoly".) UNISIST says that "the matter has been brought up again and again as the development of new information technology raises problems of proprietorship that were not envisaged twenty years ago", when the Universal Copyright Convention was developed. It complains that the need to obtain clearance for the reproduction of documents of all kinds is a barrier to the free flow of information and asks that there should be a serious examination, by all the countries adhering to UNISIST, of the question how far it would now be practicable to abandon copyright on scientific material. This is, of course, a radical issue. In journals such as *Nature* and in books of all kinds, the author is the one who holds the copyright and, although there are many circumstances in which he does not care how freely his material may be reproduced elsewhere than in the place in which it was first published, there are also circumstances when it would seem to him to be grossly unfair that free reproduction should be assumed as a right. But especially where the publication of books is concerned, the abandonment of copyright would also frequently mean the end of a great many commercial publishers. The naivety of this recommendation is yet another reason for fearing that UNISIST will have to learn to walk before it runs as hard as its prospectus promises.



## General Books

### Statistical Papers

*Statistical Papers of George Udny Yule.* Selected by Alan Stuart and Maurice G. Kendall. Pp. viii + 447. (Charles Griffin: London, August 1971.) £6.90.

GEORGE UDN YULE, who was born in 1871 and died in 1951, was certainly a peer of the statistical realm. Some knew him as one of England's gentlemen but many more as an innovator of statistical techniques and a teacher of great capability and understanding. The book by which he will be known to most (and recalled with gratitude) is the *Introduction to the Theory of Statistics*, first published in 1911 and still in substantial demand with the fourteenth edition (1950) being the fourth in collaboration with Dr M. G. Kendall, one of the selectors for the volume now under consideration. To say "review" would be impertinent as well as discourteous, therefore let it be stated that the distinguished selectors have carried out with great understanding their difficult task of exclusion: the complete bibliography shows their problem clearly.

To quote from the preface, "The reason why these twelve papers were selected . . . is that they contain work whose historical significance in the development of statistical methods is matched by its continuing importance". To the selected papers is added the account of the man written by Dr Kendall (*J. Roy. Statist. Soc., A*, **115**, 156; 1952). The selected papers fall into three groups: theory of association and correlation, epidemiological studies and the analysis of time series.

The first group (1900, 1903, 1907 and 1912) form the basis of current practice especially with respect to the difficult problem of notation. In fact, Yule's work on measures of association between attributes is still the principal reference and it is of importance to current and future generations of students that they should know that the date is not significant in any sense of being old and out of date. The selectors note that these four papers do not exhaust Yule's work in this field and an item drawn from the bibliography "On the interpretation of correlations between indices and ratios" (*J. Roy. Statist. Soc.*, **73**, 644; 1910) is of considerable importance in economic assessment.

The second group (1910, 1915, 1917 and 1920) are in the field of epidemiology but they contain the fundamental work (jointly with the late Professor Major Greenwood) on the negative binomial distribution; a form which has

very wide-ranging applications both as a logical model and as a descriptive device. Moreover the 1910 paper is an early example of what is now known as sequential analysis and the 1920 paper is an original example of mixtures of distributions upon which a great deal of more recent work has taken place.

The third group (1921, 1926, 1927 and 1945) are on the analysis of time series whereas the philosophy of serial correlation, the correlogram and the autoregressive series (as a stochastic model) is set against that of classical harmonic analysis. This step is now seen to have been vital for the understanding of economic and social activities where man's memory plays an important part.

Looking back it is interesting to see the time sequence of these three groups; interspersed in these is a range of other papers, including all Yule's work on the statistical analysis of literary style and vocabulary, which should also not be overlooked by teachers, students, research workers and practising statisticians now and in the future. The selectors and publishers are to be congratulated and commended on this volume to celebrate the centenary of Yule's birth.

W. R. BUCKLAND

### Postal History

*History of Letter Post Communication Between the United States and Europe, 1845-1875.* By George E. Hargest. (Smithsonian Studies in History and Technology, No. 6.) Pp. ix + 234. (Smithsonian Institution: Washington, 1971.) \$4.25.

ON reading the title of this book two questions immediately spring to mind: why 1845 and why 1875? The answer to the first is tied up with the trade and business rivalry between the United States and Great Britain. A British steamship line (Cunards) had become so efficient that their regular fourteen-day service between Boston, Massachusetts, and Liverpool (the "Atlantic Shuttle") had forced the American sailing packets well into second place. The British ships carried the mail, cabin passengers and "fine" freight; the Americans carried the coal. American pride was piqued and Congress was concerned about the security aspect of all the American mail to Europe being carried by British ships. In 1845 an act of Congress was passed subsidizing the United States mail packet ships, in an attempt to overcome British domination of the Atlantic service.

Why 1875? To answer this we have to go back to the time before the General Postal Union. Then a letter from the

United States to Europe could not be fully prepaid, some of the postage had to be paid in the United States and some in Europe. When a postal convention existed, accounting between the post offices of the contracting countries was established which made it possible to pay the whole postage on a letter sent or received. On January 1, 1876, France, the last of nineteen European countries to do so, placed in force the provisions of the treaty of Berne and became an active member of the GPU.

The history of the letter post communication between the United States and Europe between the years 1845 and 1875 is told in this book. It is divided into sections describing the negotiations of the United States-Bremen, Germany, Postal Arrangement of 1847; the postal treaty with Britain (December 1848); postal relations with France, and the United States-French Postal Convention of March 1857; the Prussian Closed Mail Convention; postal relations with Belgium 1844-1868; amendments in the 1860s to the above convention and the operations of the steamship lines; postal conventions effective after January 1, 1868; postal relations with France 1870-1876; depreciated currency covers and finally the postal rates between the United States and all foreign countries.

The book is obviously the result of much painstaking research, largely drawn from primary sources. It contains many illustrations of postal covers ("the cover being to the postal historian what the artifact is to the archaeologist") and is very well indexed. It makes a worthy addition to any postal historian's bookshelf.

DAVID W. HUGHES

### Old Affinity

*Affinity and Matter.* By Trevor H. Levere. Pp. xvii + 230 + 14 plates. (Clarendon: Oxford; Oxford University: London, September 1971.) £4.50.

LEVERE opens this book with an excellent chapter summarizing the major eighteenth century views on affinity. Then follow the four principal chapters chiefly concerned with setting out the speculative views of Humphry Davy, Michael Faraday, Jöns Jakob Berzelius and Hans Christian Oersted. The final two chapters respectively describe the input from the rapidly growing field of organic chemistry, and the mid-century anticipations of what became thermodynamics. The first chapter and the last two thus provide a historical setting for the author's major concern to dis

sect the intricacies of early nineteenth century metaphysical speculation on affinity.

Leveré sees "chemical affinity as a unitary concept in nineteenth century chemical theory" (page vii), but as the material is presented, it seems more a projection of his own fascination with the speculative than a believable historical reality. He devotes more pages to the Kantian metaphysics of the poet Samuel Taylor Coleridge than to the two contemporary books on chemical affinity by Claude Louis Berthollet; he treats Boscovich's point-atomism in more detail and with more sympathy than the operational atomism of John Dalton. Though such an emphasis is consonant with the author's expressed desire to move history of chemistry away from a concern with only those events that led directly to the "truths" of present day chemistry, Leveré's interpretation seems as distorted as the "Whiggish" one he is trying to replace.

Nonetheless this is an impressive book, both for what it contains and the scholarship that produced it. The empirical basis for most of the speculations about the nature of chemical forces was Davy's announcement in 1806 that electrical attraction and chemical affinity might be the same thing. Davy's own undisciplined speculations get the fullest treatment, but Leveré gathers his evidence from many scientists and much of it from manuscript sources. The sheer quantity of speculation from men known chiefly for their experimental discoveries is significant and tends to confirm the sense of uncertainty visible from other evidence at this time, a disquietude that chemistry's progress appeared to be accidental and its basis unphilosophical.

The material in this book has been previously little attended to, probably because such speculations have not been thought to have contributed to the progress of chemistry. Leveré, however, seems to believe that their metaphysical views led these men to their scientific discoveries. For example, he states that Oersted's metaphysical outlook "was eminently successful in furnishing him with theories that led to discoveries, and as such it deserves attention" (page 139). This is an enormously important claim, but Leveré hardly attempts to prove it. He does not describe the particular hypothesis that Oersted had in mind when he discovered the relationship between electricity and magnetism; he does not show how that hypothesis could have been derived from Oersted's metaphysics; he does not describe the experiment nor the relationship it demonstrated. Nonetheless, Leveré has shown that these scientists cared very deeply about their speculations, and future biographers will probably find individualized explanations for the

relation of metaphysics to discovery.

The material in this book is important and largely new. Whether or not he agrees with the interpretations through which it is here presented, no historian of nineteenth century science should neglect it.

ROBERT SIEGFRIED

## Medical Care

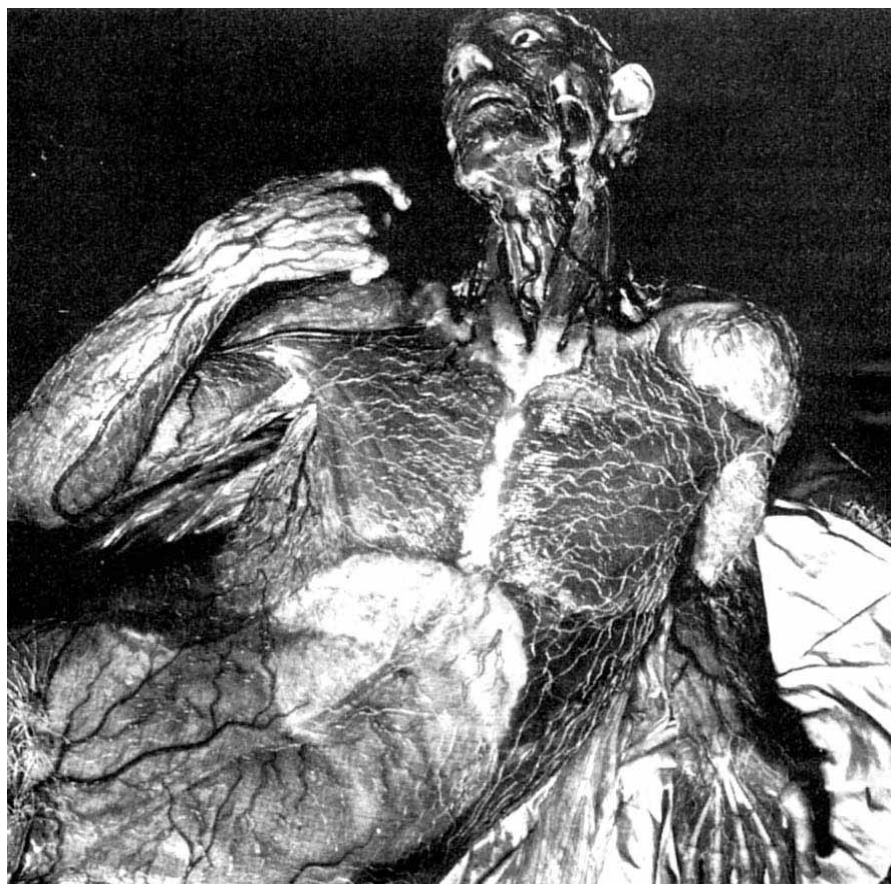
*Medical History and Medical Care: A Symposium of Perspectives.* (Arranged by the Nuffield Provincial Hospitals Trust and the Josiah Macy jun. Foundation.) By Gordon McLachlan and Thomas McKeown. Pp. xii + 244. (Oxford University: London and New York, 1971.) £3.

THIS book is the record of a symposium held in London in October 1970 under the auspices of the Nuffield Provincial Hospitals Trust and the Josiah Macy jun. Foundation. The purpose was to explore and evaluate the history of the human experience of disease in an attempt to ascertain how far such historical studies are capable of contributing to the solution of contemporary problems.

In the preface Lord Cohen of Birkenhead comments that it is far too early to assess the ultimate value of the symposium. "Perhaps as much was gained in the informal meetings with one's colleagues as in the more formal sessions, and perhaps future meetings of similar type will help to illuminate the persisting shadows."

The book is, in effect, a series of essays, each of which is followed by a summary of the original discussion on it. They comprise two papers by Thomas McKeown, on a sociological approach to the history of medicine and a historical appraisal of the medical task; changes in the supply and characteristics of American doctors in the twentieth century by John Z. Bowers; historical trends and future prospects in public health by George Rosen; evolution of medical practice by John Brotherston; historical discontinuity, hospitals and health services by Paul J. Sanazaro; the influence of medical technology on medical services by Bernard Towers; on measuring economic benefits of health programmes by Rashi Fein, and a contemporary view

## Science in Florence



One of a collection of anatomical models made in wax by Clemente Susini (1754–1814) which can be seen in the Specola Museum, Florence. This city has a history of scientific achievement dating from Etruscan times which is recorded and profusely illustrated in *La Scienza a Firenze* (edited by G. Chiarelli for the Florence Tourist Board. Pp. 205. Edizioni d'Arte Il Fiorino, Florence, 1971).



of the historical influences on medicine by Henry Miller. References are appended to each discussion.

Dr Gordon McLachlan completes the postscript by saying that the symposium may prove to be the modest beginning of a movement towards a better understanding of problems common to all countries. It is by this standard that the book should be judged.

There were fourteen British and ten American participants in the seminar, most of whom were medically qualified, and the book, not unnaturally, reflects this pattern, for it discusses Anglo-American medical history and medical care. As the theme develops, from the evolution of medical practice to the measurement of economic benefits of health programmes, those with field experience in "developing" countries may find the gulf between the problems and resources there and those of the wealthy and sophisticated societies under scrutiny almost terrifying. It is going to be difficult to find common ground for future discussions, for the solutions must also be different.

The quality of the essays is uniformly high and the reader is not unduly distracted by the transition from English to American styles of writing. Some are controversial, notably those by McKeown. Others are monographs, complete in themselves, and Brotherton's essay on the general practitioner is an outstanding example of this. So also is Rashi Fein's contribution on the measurement of economic benefits of health services.

The purpose of the final essay by Henry Miller was to review those that preceded it and to seek pointers to forward planning. It is an amusing and at times devastating commentary on the proceedings which should be read in full, but it can be epitomized by his verdict that: "If anything is clear from this meeting it is that we cannot predict medical developments during the next fifteen years. How can we in all conscience return to our hospitals and put forward plans that claim to meet the needs of fifty years ahead?"

In short, this book is an authoritative record of the current anxieties and problems in the field of medical care in Britain and the United States of America.

A. LESLIE BANKS

## Bioethics

*Bioethics: Bridge to the Future.* By Van Rensselaer Potter. Pp. xvii+205. (Prentice-Hall: Englewood Cliffs, New Jersey. March 1971.) \$5.95 cloth, \$3.95 paper.

THERE is a need, apparently felt by a number of individuals who have had a long and productive research career, to

gather together in one place their thoughts on a variety of disparate topics. It is an easy way to build a book out of past material, labour saving and doubtless ego-gratifying for the author; presumably profitable for the publisher. If it is dressed up with a new and trendy title, the unwary will believe that the book is really a book and not a sort of *paella* made with the left-overs. There is of course nothing against reissuing one's minor or even seminal thoughts in this way; one recent book containing the collected views of a respected Chinese scientist-cum-political leader has reputedly sold into the millions. But I do feel that publishers should warn prospective purchasers that they should not buy on the assumption that here is a new book.

Disregarding, then, the pretentious title, what is one to make of this *pot-pourri*? Dr Potter is evidently a likeable, moral and concerned man; his thoughts radiate the common sense of the American liberal, convinced that "we need to compete with the communists in the field of ideas and not merely in the production of corn, hogs and missiles". His contribution to the competition is, on the one hand, a straight down-the-line defence of objectivity and scientific progress, conducted, however, without the clarity of Jacques Monod's *Le Hasard et la Nécessité*, and, on the other hand, an uneasy but undefined feeling that not all is well with the concepts. To bolster them comes "bioethics", a *mélange* of Teilhard de Chardin and ecological concern. If Dr Potter does not go all the way with those *Nature* has in the past described as the "econuts", and he is far too sound a man to thicken the air with doomful prognostications, he does believe in biological responsibility. . . . "An urgent task for bioethics is to seek biological agreement at the international level."

This is all very well intentioned but rather far from the real problems of contemporary society, which are not so much the accidental as the deliberate consequences of the application of science and technology. Thalidomide, happiness and Pandora's box all get references in the index. The Garden of Eden and the ethical revolution are there, but Vietnam and the military-industrial complex do not appear; the problems of the third world countries are encapsulated in the limpid but scarcely acceptable conclusion that ". . . the United States, Western Europe and Russia are the materialistic giants while India is a country with essentially the religious attitude towards progress". One fears that Dr Potter's irony was at best unintentional.

There are some good things in the book, though unexpected simply insofar as it is difficult to predict anything about

its contents; two interesting chapters on an optimum environment for man, which draw on the author's own research; a parenthetical sideswipe at Jansenism; and a brave and intriguing first chapter. This chapter, although entitled misleadingly "Bioethics", is an attempt by Dr Potter, under the heavy and acknowledged influence of T. S. Kuhn's views on paradigms in science, to extract what he believes to be the twelve central paradigms of contemporary mechanistic biology, ranging, in hierarchical order, from those concerning molecular systems, to those of physiological adaptation. Whatever the merits of the Kuhnian analysis, this particular exercise is worthwhile, and could well be built in conceptually to the training of biologists, who need to be taught more clearly the epistemology of their subject as well as its "facts".

STEVEN P. R. ROSE

## Nonscience

*Nonscience . . . or How not to Rule the World.* By Brian J. Ford. Pp. 206. (Wolfe: London, September 1971.) £2.00.

SOME of the elements of which *Nonscience*, by Brian J. Ford, is composed made me laugh aloud; others exasperated me to about the same degree. Its publishers promise us a satirical look at our age, and Mr Ford makes his intentions clear immediately. This is an age when "accepted codes of technological betterment may replace our notions of humanity"; but there is no sense in wanting to abolish technology, nor virtue in being anti-science. The enemy to be driven out is "pragmatic, self-fulfilling, narcissistic Nonscience", which Mr Ford defines in terms of its mechanics, its effects, its language—the stock-in-trade of a new breed of individual, "the expert".

The essential feature of the expert is absolute dedication to data (rather than to new ideas and imagination) which he uses omnisciently to substantiate whatever case happens at the time to promote his own standing, usually by its getting him into the public eye. The sarcasm may be pretty heavy, but Mr Ford has a point. What exasperated me was the stance he takes for establishing it in the first place—too chip on the shoulder; too studentish, as exemplified by students who get into the public eye by posturing as self-appointed members of a depressed class. One of Mr Ford's early swipes is at the people who do technical jobs in the Department of Trade and Industry. It matters less that he's wasting his energy than that he's showing an absence of grasp on how the world works. This will make his swipes seem the less to be worried about by the people who actu-

ally work the world, when he comes, as he should, to swiping at them.

However, Mr Ford is witty and enjoys playing with words. I liked, for example, his transformation of "integrity" into "considered circumspection". He makes his own contribution to the language of Nonscience (which only the expert can understand, of course): for being concerned with what gets you into the public eye he coins the word "fashionism" — in true Nonscience language, "quasi-notional fashionistic normativity". (No prize for recognizing the true language of the social sciences!)

The body of the book consists of chapters along the way, for the expert, from training to fame. They tick off some highly recognizable ploys for scoring in examinations, for getting a PhD thesis accepted, for writing an eye-catching first paper and so on. Nonsciencemanship or sciencemanship, depending on which way you look at it. There is a historical section, which to me seemed rather strained, where he demonstrates that discoveries and inventions made by men whose names every schoolboy knows were made earlier by other men whose "quasi-notional fashionistic normativity factor" (quFN Factor) was just too low. Coming to some of the experts *de nos jours*, he has very tasty things to say about, for example, Professor Christiaan Barnard and Dr Desmond Morris. (The piece on "transplant fashionism" is deliciously well researched.)

The book ends in a hail of miscellaneous swipes, some of them about as significant as shying at coconuts, others — for example, on current sex education — based on thoroughly sound human feeling. After that there's an examination paper which tests the reader's capacity for making the grade as an expert. To sum the book up: for my money there's too much of it, too many shallows between the occasional deeps. That's on the one hand. On the other, it would be ungracious of me not to say it made me laugh and Mr Ford's point is worth taking. And if he is lucky, he may plant his word "fashionistic" in current speech. WILLIAM COOPER

## Arms Control

*Impact of New Technologies on the Arms Race.* Edited by B. T. Feld, T. Greenwood, G. W. Rathjens and S. Weinberg. (A Pugwash Monograph.) Pp. xvi + 379. (MIT: Cambridge, Massachusetts, and London, April 1971.) \$12.50; £5.85.

Most conference reports make for rather loose-jointed books and this one, a symposium on technical aspects of the arms race, is no exception. However, the tenth in a series of International

Pugwash Symposia, held at Racine, Wisconsin, in mid-1970, features some chapters of very direct bearing on control of the arms race. Two chapters in particular merit attention—the first on ballistic missile guidance, and the second on the restriction of research and development. These two issues are of paramount importance in the control of modern armaments and illustrate the grave difficulties which seem to insinuate the Strategic Arms Limitation Talks (SALT).

D. G. Hoag, director of MIT's Apollo Guidance and Navigation Program, has given a very authoritative 105 page exposition of missile guidance technology. The significance of this ICBM development is lucidly stated as follows:

"The development of high accuracy in ballistic missiles may be interpreted as an intent to achieve a first-strike capability by which an aggressor, by firing first, can destroy his adversary's weapons and ability to respond in retaliation. The development of high accuracy coupled with a multiple-warhead capability may appear even more sinister."

The US development and deployment of MIRV-missiles (multiple, independently targetable, re-entry vehicles) thoroughly confuses the arms race, raising both quantitative and qualitative problems for arms control. How many warheads (MIRVs) are aboard each missile? What is the quality of the missile guidance system?

Any dependable answer to the first question must inevitably boil down to on-site and, in fact, in-silo inspection. It is complicated by the fact that the Soviets have yet to deploy any MIRVed SS-9s (their heavyweight inter-continental ballistic missile); thus the inspection problem is asymmetric.

As for the question of warhead accuracy, the answer is filled with dilemmas as illustrated by Hoag's observation: "More accurate systems are almost certainly coming, and when they do come we may not know for sure that they have arrived". One could go a step further and assert that, since quality cannot be inspected, an enemy is driven to the worst assumptions. Fear of a first strike overdrives the arms race and the inability to delimit quality of a strike system sorely taxes the ability of those who would reach agreements on strategic armaments.

Arms controllers are always nervous about what may pop out of the research and development oven. Any agreements they may promote may be sabotaged by weapons zealots, who prematurely disclose advances which may take place. Jan Prawitz of Sweden's National Research Institute of Defence contributes an incisive com-

mentary on restricting research and development in which he concludes:

"There is, therefore, no general way of controlling military R and D, as this control would sometimes have to deal with the intentions and thoughts of individual scientists. In specific areas, however, practical measures might be envisaged."

He suggests one—the nuclear test ban (where, alas, the treaty has not slowed the clock of weapons technology) and another—a MIRV test ban. The latter is no longer feasible because of the asymmetry in Soviet and US testing and it seems that MIRV is a runaway technology which will elude the grasp of the Strategic Arms Limitation Talks.

The United States appears to be racing itself, not competing with the Soviet Union, as it seeks to exploit military technological opportunities.

RALPH E. LAPP

## Abstracts

*Abstracts and Abstracting Services.* By Robert Collison. Pp. 122. (Clio: Santa Barbara, California, and Oxford, May 1971.) \$4.75.

THIS book gives a fairly wide survey of the history and current practice of abstracting, and deals with the nature and style of abstracts, the writing, editing and indexing of abstracts, as observed in practice, and to a lesser extent their publication. There is a special chapter on the early history of reviews and abstracts; present services are also rapidly scanned, and there is an appendix of some five hundred titles; there is also a useful reading list of papers on the subject. There is a two-page chapter on the possibilities of mechanizing abstracting. The index covers mainly authors and institutions, but otherwise not more detail than section headings.

The writing is straightforward and clear, although in parts of the book the descriptions are far too much like a catalogue, instead of an informed discussion. The examples, and hence the rather limited discussions, are largely taken from the humanities and social sciences, with relatively few references to scientific and technical journals (though the appendix lists more). Considering that the big developments in modern abstracting, the major demand, and the outstanding examples of abstracting services have been in the scientific and technical fields, the imbalance of the book's approach becomes increasingly obvious. *Chemical Abstracts* is given three or four minor mentions (chiefly in the "early history"), and its innovations in production and outstanding indexes, as well as the tape versions, are not described at all. POST is listed, but not CBAC. *Bulletin Signaletique* is mentioned, but not the



*Referativny Zhurnal*. Subject indexing is given scant attention, although it is the essential tool for retrospective searching, especially since computerized indexing, mentioned several times, is not yet at all efficient.

The book is in no way a textbook and those wishing to learn how to abstract and produce an abstracts journal will have to look elsewhere. As Mr Collison's intention is, however, to provide an analytical survey of the subject, this is not a fault, but he does include some advice in places in the first half of the book, and then betrays at once his lack of any authoritative knowledge of the techniques of the subject. Abstracting is not really in the field of librarianship. Thus, on page 3, Mr Collison defines an abstract as "the terse presentation, in (as far as possible) the author's own language, of all the points made, in the same order as in the original piece of primary document information". In my opinion (after years of teaching abstracting) this is dangerously misleading. In most, if not all, subjects it is important to aim for some consistency in abstracting style, and of course to standardize terminology (for example, of units, chemical compounds), especially to help subsequent indexing, and it is also always desirable to present the informa-

tion in a simple logical order (correctly given on page 27) so that communication with the reader is clearly established, whereas many papers and authors' styles are far from this ideal order.

In discussing the vexed question as to who should abstract, the existence of the trained abstractor is ignored. Advice on the writing of abstracts includes apparent approval, in some cases, of a telegraphic style not acceptable in good journals. There is a suggestion that a slanted abstract, considered somehow as different from a discipline-oriented or a mission-oriented abstract (and this latest American jargon is not helpful), emphasizes only a subject-oriented portion of a document; this, again, is misleading: a slanted abstract is desirable in any special subject field, and although it should emphasize those subject interests it should still give an adequate, though brief, statement of other parts of the paper. The advice on abstracting technique — reading the paper two or three times, underlining or making marginal notes of important passages (and thus ruining the journal), and then "constructing a narrative" — will obliterate the desired logical order of information and tend to make the abstracter copy the author's style and

language. Consider the effect when abstracting a German paper! The better technique is first to skim the articles, bearing in mind the desired logical order (purpose, methods, results, conclusions, side issues) and identifying those points; the whole paper can then be read somewhat selectively, to note more efficiently the necessary detail; the actual writing is thus greatly eased.

As regards the citation of the title, authors and journal references, examples of different practices are quoted, but there is no suggestion of a suitable standard. Incidentally, the problems of abstracting patents are entirely overlooked. There is also a strange suggestion that an abstract should never include a negative statement (though such a statement may be as important as a positive result) because a computer could not then deal correctly with keyword selection from a taped record. So much the worse for computerized indexes! Many other minor matters might be raised, but these examples are perhaps sufficient for my purpose. The criticisms are indeed made more in sorrow than in anger. A more or less historical survey is interesting; it is an accurate treatment of the many and varied current problems which is urgent. It must be based on wide experience.

J. FARRADANE

## Physical Sciences

### Relativity and Reality

*Theory of Relativity Based on Physical Reality*. By L. Jánossy. Pp. 317. (Akadémiai Kiadó: Budapest, 1971.) £5.

*The Conceptual Foundations of Contemporary Relativity Theory*. By John Cowperth Graves. Pp. xi + 361. (MIT: Cambridge, Massachusetts, and London, May 1971.) £7.

REVIEWERS perform an important function and should not be irritated. Jánossy's book has no index (a serious omission). Graves's book has a vile purple jacket, with white printing on it (hard to read); the headings of the pages are put at the foot (a silly gimmick); the notes (which should be footnotes) are collected at the end, and how this made me curse! For example, I am thrilled to read on page 53 that Wheeler gives me credit for introducing the notion of a "plan". What plan? Ha, there is a note 17. But where? Feverishly I turn to the back of the book. Frustration! I must know the chapter. Back to page 53, but it does not say what chapter. Leafing back, I find its number. Now once again to the back, and the required information is mine. But, to be frank,

I am by that time frustrated and angry at this foolery. So two pieces of advice to publishers: (a) if a scientific book is worth printing, it is worth indexing, and (b) the place for headings is at the head and the place for notes at the foot of the page. I have got so cross over all this that I must cool off for a moment before proceeding with my responsible task.

These two books are about relativity, but there the resemblance ends. Jánossy's style is simple, concise and direct, as fits a man interested in "physical reality". Graves floods the page with philosophical polysyllables, and one gets the impression (perhaps wrong) that what he has to say could be said much more briefly. Jánossy's approach to relativity is very individualistic; he insists on his specially devised notation and terminology, but in the end one finds him safe on the mountain-top, refreshing himself with old wine in new bottles, labelled (spuriously, I think) "physical reality". Graves, in the manner of philosophers, is not really interested in getting anywhere and floats around the mountain in a helicopter, looking at the view from many aspects, the accompanying music being supplied by a host of other philo-

sophers ("As A. has remarked . . ." or "As B. has pointed out . . ."). At the end, having thoroughly inspected the Einsteinian peak, the helicopter proceeds towards Wheeler's geometrodynamics, an object which is to be described as rock or cloud according to the personal assessment of the observer.

But, to quit metaphors (as if one ever could), Jánossy tells how he got acquainted with relativity at a comparatively early age; he had some difficulty with some of Einstein's concepts, but, being young and enthusiastic, he convinced himself in the end that he could understand those concepts; in trying to explain the theory to others, he learned the "language of relativity" and gradually "got used to" the theory. But a certain "bad feeling" remained; in the end he became convinced that from the philosophical point of view the concepts had to be changed; since about 1950 he struggled with the problem of reformulating the theory; this book represents the result of his deliberations.

Very fairly and honestly stated. Who of those who now call themselves "relativists" did not have to struggle hard with relativistic concepts? I, too, had that "bad feeling", and it was

around 1950 that I, too, tried to sort things out. By adding a little here and a little there to what I learned from books and discussions, I have arrived at a reasonably comfortable state in the sense that, when confronted with any relativistic problem, I know how to begin to think about it, even though (most probably) I cannot solve it. It is an additional (and important) comfort that, although my concepts may be influenced by personal quirks, they are not essentially different from those in the minds of most relativists. Communication is possible, if difficult at times.

Jánosy frightens me. All the old ghosts that I have so laboriously driven out haunt his pages. To him, it appears, such concepts as distance, time, velocity, rigid body (experiencing Fitz-Gerald-Lorentz contraction) and worst of all the ether—are basic and acceptable concepts. Yet, with these intolerable aids, there he sits on the mountain-top. I can view his climb only through a glass darkly, for it is veiled by unusual symbols and words (the metric tensor becomes the propagation matrix, an event becomes a four-point). The question is this: Can anyone but Jánosy understand Jánosy? If a forest of affirmative hands shoots up, I shall blush with shame at my ineptitude. If none, then at least he has had his catharsis and has satisfied himself.

Cutting through the philosophical jabberwockery, I feel at one in essentials with Graves, so much so that I can pick on trifles (or are they?) to query or criticize. Thus (page 314): "The field equations are nonlinear, a situation unprecedented in either classical or quantum physics". Nonsense: Euler's hydrodynamical equations are nonlinear. Next (page 194): "... the speed of light is not constant if there are gravitational fields". A meaningless statement unless the word speed is meaningful—presumably it means distance divided by time. Yes, but what is distance? Schoolboys may lay "rigid" measuring rods end to end, but in fact (real, practical fact) an accurate measurement of distance is a measurement of time. Nature offers us, not two basic instruments of measurement (rods and clocks), but only one (the clock—atomic clock, if you like). The excellent index in Graves's book has about half a column devoted to geometry, but its excellence is marred by the omission of chronometry, which is what relativity is all about. Once this is realized, the whole subject is flooded with light (in more senses than one), but Graves gives the matter only a passing reference (page 160), as one might refer to one of the inventions of the White Knight. Perhaps it would be clearer, ontologically, epistemologically, jabberwockologically, if I said it three times: There are no measuring rods. There

are no measuring rods! There are no measuring rods!!

My thousand words are running out, and I shall not be able to comment (with approval) on the long discussion about the two sides of Einstein's field equations, nor (with disapproval) about the geodesic principle for test-particles, nor to express mild wonder at the view that there has been great progress in relativity since 1955 (it really depends what you mean by progress). What I shall always remember about Graves's book is that it set me to re-reading, after many years, Laurence Sterne's *Tristram Shandy* (how this came about is too devious to describe). For gravity has a second meaning ("A mysterious carriage of the body to cover the defects of the mind;—which definition of gravity, *Yorick*, with great imprudence, would say, deserved to be wrote in letters of gold"), and it seems to me that God can afford to laugh, Newton and Einstein to titter occasionally, but the mere acolyte is afraid to smile for fear of spilling a drop of the sacred oil.

J. L. SYNGE

## Papal Galaxies

*Nuclei of Galaxies*. Edited by D. J. K. O'Connell. Pp. xi+795. (North Holland: Amsterdam; Elsevier: New York, 1971.) Hfl. 134.00; \$40.00.

FROM time to time in the history of any science, there arise so many paradoxes, discrepancies, and unexplained phenomena that the deepest foundations of that science are challenged. This is happening in extragalactic astronomy. Regions of space only a few light years across are found to radiate much more radio, infrared, optical, and X-ray energy than our entire galaxy (the diameter of which is about 100,000 light years). This luminosity often fluctuates considerably over periods of several years or, in some cases, during one day. Galaxies exist which are apparently connected by a bridge of luminous matter, yet their redshifts would imply velocities which differ by more than  $8,000 \text{ km s}^{-1}$ . There is increasing evidence that groups of galaxies fly apart with positive total energy. How could they have formed?

The most likely candidates for sources of these strange phenomena are the nuclei of galaxies. In our own galaxy, this central core may contain more than  $10^7$  stars  $\text{pc}^{-3}$ ; it is not particularly active and other galaxies may have denser nuclei. Often these contain gas clouds in violent motion, emitting abnormal amounts of ultraviolet light. Some of these nuclei are observed to eject at least  $10^{-6} M_{\odot}$  of gas and stars, others may eject coherent objects which become radio emitters or even new galaxies and quasars. In fact, there are

substantial similarities between the properties of active galactic nuclei and those of quasars. Many new observations in the last few years have made the time ripe for an extended study of galactic nuclei.

This book records the third international astronomical "study week" held by the Pontifical Academy of Sciences in the Vatican. Like its predecessors on stellar populations and interplanetary cosmic rays in 1957 and 1962, this will be a major, influential and lasting contribution. As befits its subject, the book itself is unusually massive. The soft-cover edition, printed by the Vatican, weighs  $4\frac{1}{2}$  pounds. The hard-cover copy, lacking the history of the Pontifical Academy and the Papal address, printed on somewhat cheaper paper with a slightly unstable binding by North Holland, is the less massive version.

There are thirty-one papers organized into five main sections. First, an introduction by V. A. Ambartsumian summarizes the types of activity in nuclei. A striking form of this activity is jets having small bright blue condensations similar to the nucleus from which they seem to emerge.

The second main section contains fifteen papers on observations and their interpretation. Most of these are excellent reviews, with many new results, on the following specific topics: the optical morphology of Seyfert galaxies (Morgan *et al.*), the stellar content and evolution of galaxy nuclei (Spinrad), the optical spectra of galaxies and QSOs (Sargent, also E. M. Burbidge), physical conditions in the nuclei of galaxies and QSOs deduced from line spectra (Osterbrock), absorption redshifts in QSOs (McCrea), infrared emission of galaxies (Low), compact radio sources in nuclei of galaxies (Kellerman), expansion models of eruptions in quasars and radio galaxies (van der Laan), optical properties of nuclei (Sandage), comparison between the properties of the nucleus of our galaxy and that of M31 (Oort), velocity dispersions and discrepant redshifts in groups of galaxies (E. M. Burbidge and Sargent), space distribution and luminosity functions of QSOs (Schmidt), space densities and time scales for Seyfert galaxies, radio galaxies, and QSOs (Schmidt), and quasar statistics for Lemaître cosmologies (Salpeter).

The third section, on theory, contains discussions of the non-thermal emission and ejection of matter (G. R. Burbidge), the dynamical evolution of dense spherical star systems (Spitzer), massive rotators and spinars (Woltjer, also Morrison and Cavaliere), rotation and pulsation periods for pulsar models of quasars (Fowler), formation and evolution of black holes which accrete gas (Lynden-Bell), mechanisms for jets



(Wheeler), and a new theory of relativistic compact objects (Hoyle).

Observational cosmology and galaxy evolution are discussed in the fourth section which reviews the age of galaxies and globular clusters with a view to finding the Hubble constant and deceleration parameter (Sandage), the evolution of radio sources (Rees), the curious mystery of  $\log N - \log S$  (Hoyle), the X-ray background radiation (Friedman), and the X-ray and microwave background (Morrison).

The final section contains excellent summaries of the observational (E. M. Burbidge) and theoretical (L. Woltjer) positions, a list of observational and theoretical conclusions and useful suggestions for future work, and name and subject indices. The total ratio of observation to theory is about 3 to 1. There is also a reasonable amount of recorded discussion (about one-fifth of the book) of the papers. The quality of editing is high, although there are rare slips as when a discussion refers to previous comments or diagrams which are not recorded.

The lists of agreed conclusions resemble some of the diplomatic communiqués which emerge after heads of state meet in the countryside. The general style of the observational conclusion has the form: There is strong (increasing, extensive) evidence that . . . On the other hand, the theoretical conclusions leave the impression . . . It might be possible that . . . Most of the theoretical conclusions are really questions, and this indicates the present uncertainty surrounding the foundations of the subject.

Indeed, in his summary, Woltjer remarks: "Even though no strong controversy has developed at this conference it is very clear that, if we try to write down a set of definite conclusions from the theoretical point of view, many of our old disagreements will come to the surface very quickly. This meeting has been extremely successful in part because some of our differences have been set aside for the moment. . . ." The more conventional side of this controversy believes that the nuclei have formed by contracting from a more diffuse state and that the observations can be explained by conventional physics and standard general relativity. The alternative view is that the nuclei preceded and produced their less dense surroundings, and new laws of physics are required.

At present, it is much a matter of personal judgment whether one believes conventional physics can explain these discoveries. Do we need new laws of the redshift, new methods of releasing vast amounts of energy? Or are we just too unimaginative in applying existing theory? These questions are so difficult because the observations only supply very limited information about

dozens of interacting physical processes. After observing extragalactic phenomena for many decades, it is amazing that we know so little. Yet it is even more amazing that we know so much.

WILLIAM C. SASLAW

## Background to Plasmas

*Sources of Plasma Physics*. Vol. 1. By H. S. Green and R. B. Leipnik. Pp. 630. (Wolters-Noordhoff: Groningen, 1970.) 118 Dfl.; £3.75; \$35.

THE primary object of this work, according to the authors, is to bring together in a single volume discussions of the mathematical foundations of statistical mechanics, the principles of electromagnetism, fluid dynamics and the kinetic theory of gases in order that a second (future) volume on plasma physics may be properly understood. Even in a book of this size, such an ambitious project must of necessity leave out substantial aspects of each of these subjects, particularly when more than a quarter of the available space is devoted to such diverse "mathematical method" topics as measure and probability theory, vector spaces, Fourier analysis, vector analysis, differential equations, calculus of variations and thermodynamics. Most university undergraduates and research workers have occasion to study these topics in depth before approaching the more formidable fields of magnetohydrodynamics and plasma dynamics. It is very difficult, if not impossible, to give a comprehensive account of all the relevant techniques of mathematical physics in one book. Certainly an extensive knowledge of pure mathematics is required before the relevance of the highly condensed appendices at the end of this book can be appreciated.

Before studying plasmas, almost all students will have attended substantial courses on electromagnetism and relativity, quantum mechanics, hydrodynamics and fluid mechanics. Furthermore, in Europe, such courses will have been given using SI units, whereas this book is written in CGS units and as such will have little appeal to non-mathematicians. After a prolonged struggle to master the SI system, I have no nostalgic longing for Gaussian or other discarded units and I find this return to the past, presumably intended for the benefit of American readers, annoying and confusing, particularly because the book is aimed at students of physics and engineering.

Many of the more difficult problems, for example those on drift analysis, and the publications of Russian authors receive little prominence but perhaps this will be resolved in later volumes. In a work of this kind a fairly detailed historical account is always of value. In

this field there is much to be learnt from the lessons and mistakes of the past. For example, has any research worker in magnetohydrodynamics failed to rediscover Ferraro's law of isorotation? Here the historical aspect is played down somewhat and is replaced by concise discussions of the fundamental physical aspects and formulation of the associated mathematical problems.

This emphasis on physics, however, does not permeate throughout the whole book. The mathematical treatment of many problems is very sound but physical interpretations of mathematical results are very brief or non-existent.

It will be interesting to see how a prospective research student, in his first year after graduation, copes with this volume. It is always difficult to assess parts of a multivolume work in isolation, and so after the appearance of the second volume, which deals with a theoretical formulation of plasma physics, it will be possible to give a more precise estimate of this book.

C. PLUMPTON

## Molecular Spectra

*The Spectra and Structures of Simple Free Radicals: an Introduction to Molecular Spectroscopy*. By Gerhard Herzberg. (The George Fisher-Baker Non-Resident Lectureship in Chemistry at Cornell University.) Pp. xi + 226. (Cornell: Ithaca and London, 1971.) £5.25.

IN the preface to this book the author states that for forty years he had wanted to write a small book of some 200 pages dealing with molecular spectra and structure. However, as all spectroscopists know, during this period he did write a magnificent three-volume monograph of 2,000 pages. Now, at last, he has fulfilled his early ambition, and provided us with a short account of molecular spectra and structure. The role of free radicals is shown by the fact that wherever possible he has chosen the spectra of radicals rather than of stable molecules to illustrate his argument. The result is that we have an effectively complete account of the spectra of free radicals.

This is as it should be. For no one has contributed more than Herzberg to the understanding of these systems; and in many situations, on account of their very short lifetime which prevents any normal study by conventional chemical methods, this is the only way of studying their geometrical structure. How very right it is that his influence in this field should now have been recognized by the award of a Nobel Prize.

It is a strange story that we read. Thus the first chemical detection of polyatomic free radicals, for example

$\text{CH}_3\cdot\text{C}_2\text{H}_5$ , goes back to Paneth in 1929, but it took another thirty years before their ultraviolet spectra could be unambiguously obtained. Yet the study of radicals such as  $\text{OH}\cdot$ ,  $\text{C}_3$  and  $\text{SiC}_2$  is of first-rate importance for the understanding of stellar atmospheres, and the spectra of comets are almost wholly free-radical in character.

All this, and a good deal else, is set out in this little book. There are chapters on diatomic radicals, linear polyatomic ones, non-linear polyatomics and a discussion of dissociation and predissociation. Throughout, the argument is illustrated by a superb collection of experimental spectrograms. As one would expect from someone with the experience of the author, there is a complete control of the material; the account is crystal clear, and there is an admirable index.

It almost seems ungracious to cavil about any omission; but the complete neglect of any account of electron spin resonance does leave a gap in the story. For although electronic and rotation-vibration spectra can be made to give us information about the size and shape of a radical, ESR can tell us about the charge distribution. The two types of experiment supplement each other (though, where a full resolution is possible, Carrington has shown that almost all the information that we can want about a gaseous radical can in principle be obtained from its ESR spectrum).

The Fisher-Baker lecture series at Cornell has provided some very fine volumes. The present set is in good company, and it deserves its place.

C. A. COULSON

## Chemical Plant

*Chemical Plant Simulation: an Introduction to Computer-Aided Steady-state Process Analysis.* By C. M. Crowe, A. E. Hamielec, T. W. Hoffman, A. I. Johnson, D. R. Woods and P. T. Shannon. Pp. xiv+368. (Prentice-Hall: Englewood Cliffs, New Jersey, July 1971.) \$15.59.

THE evaluation of process plant usually involves complicated and extensive procedures containing lengthy, repetitive and tedious calculations. The advent of easily accessible digital computers was therefore a boon to chemical engineers who rapidly adopted them for process design, and later for simulation of the behaviour of plants in operation. Indeed effective simulation of plant is not feasible without the use of a digital computer. The useful result is that now a wide range of possible designs and operating modes can be investigated and searched for their optimum cases.

This book is concerned primarily with digital simulation of chemical plant for the purposes of steady state process

analysis. It introduces and explains the techniques involved and points out the value of such an analysis through a real example, by developing in detail a complete digital simulation of an existing sulphuric acid plant, using a particular simulation language called PACER. Inevitably this requires the use of much conventional chemical engineering, especially where it is being shown, in the middle chapters of the book, how digital models of the individual units making up the plant are constructed. The core of the work lies, however, in the earlier chapters where the methods used to link individual models into a complete simulation are described. It is here that it is seen that the approach needed for the simulation of a complex of plant is necessarily more ordered and disciplined than is common in process design, and that this quantification of what is usually done intuitively should be used more often in design.

The book is well planned, well written, sensibly arranged and supplied with clear diagrams. Several of the authors are known for their work in other fields of chemical engineering and it is significant of the importance of this kind of process analysis that they have got together with a chemical company and a computer manufacturer to produce this excellent result. I consider that this is an important textbook not only because of its useful contents but also because it in effect suggests that this subject, up to the present time the preserve of specialists, should now become a familiar procedure to all chemical engineers.

W. SMITH

## Strengthened Solids

*Strengthening Methods in Crystals.* (Elsevier Science Series.) By A. Kelly and R. B. Nicholson. Pp. 627. (Elsevier: Amsterdam, London and New York, 1971.) £12.50.

MANY of us, I suspect, look at edited books with suspicion, since far too often they lack a systematic approach or seem out of balance, especially when the editors are not contributors. This book avoids these pitfalls and no doubt benefits from the fact that the editors are recognized authorities themselves in the field of strengthening mechanisms in solids. They have contributed chapters themselves and the book is given coherence by the informative introductory and summary texts that they have written, which set the theme and mark the achievements.

The book, which comprises some nine main chapters (more than 600 pages) all written by authors of considerable standing, is aimed principally at specialists in the field. The first chapter, entitled "Dislocation-Particle Interactions", which takes up one-fifth of the book and is written by L. M. Brown

and the late R. K. Ham, is a clear and detailed exposition of present understanding of the ways in which hard and soft particles in a matrix can strengthen a solid. There follows a very informative account by M. F. Ashby of deformation mechanisms in two phase alloys with phases of differing yield stress. The chapter by N. S. Stoloff deals with the strength of ordered crystals and intermetallic compounds, and J. D. Embury deals with strengthening by dislocation substructures in the sixth chapter. The interesting account by J. W. Christian on "The Strength of Martensite", in the fifth chapter, reflects the advances in the knowledge of the behaviour of dislocations in alloys. Precipitation strengthening of ceramics is covered by G. W. Groves in the seventh chapter, in which, due to the nature of ceramics, the effect of particles on fracture behaviour is introduced.

During the past decade there has been a tremendous upsurge of interest in fibre reinforcement, and A. Kelly (an editor of the book) introduces this topic by dealing initially with the conditions in which fibre and matrix interaction results in matrix strength modification and leads on to the well established "mixtures rule" for strength where the interaction is less apparent and a major preoccupation is with toughness mechanisms.

The book is rounded off by two chapters by G. J. Davies and R. B. Nicholson (an editor of the book) respectively, which deal with the development of strong microstructures from the melt and the solid state. These interesting chapters show in some measure the extent to which understanding and experience (technology) can be rationalized.

The emphasis in the book is on metals, ceramics and their alloys; polymers get only a brief mention. One is tempted to say that a chapter on polymers would have been attractive, but its absence in no way detracts from the scientific value of the book. The individual authors cover their subjects comprehensively and in depth, perhaps inspired by the editors' own efforts. The book demonstrates very strikingly, I feel, the tremendous advances in our knowledge in this area that have taken place during the past fifteen years or so; this progress has been facilitated by the development of electron microscope techniques, which have led to such fruitful microstructural investigations. These advances have also brought maturity into the subject insofar that discussions of strengthening mechanisms are less polarized, as demonstrated, for example, in the conclusions to the chapter by Brown and Ham, where it is made clear that the contribution of various mechanisms, such as mechanical strengthening, order strengthening



and so on, depends intimately on the alloy system under investigation.

In summary, the book, which is well produced and contains microstructural illustrations of a high standard, will be of great interest to experts in the subject and will find its way into all the relevant scientific and technical research libraries. I suspect personal purchase by working scientists will be inhibited by the rather disturbingly high price of £12.50.

J. E. BAILEY

## Spectral Analysis

*Analytical Spectrometry.* By L. de Galan. Pp. viii+279. (Adam Hilger: London, September 1971.) £6.

THIS book is based on a course "intended for the general analytical chemist" and is an updated version of the 1969 Dutch edition. The author is at the Technische Hogeschool, Delft.

The word "spectrometry" is used in its most general sense and includes mass spectra as well as the spectrum of electromagnetic radiation from  $\gamma$ -rays to mm waves. More or less all "spectrometric" methods in general use for structural, qualitative and quantitative chemical analysis are included—molecular absorption (ultraviolet to infrared), atomic absorption and emission spectra, X-ray absorption and fluorescence,  $\gamma$ -ray emission of elements activated by neutrons, crystal structure by X-ray diffraction, and nuclear magnetic resonance spectroscopy. Notable omissions—presumably considered to be in insufficient general use—are Raman spectra, electron spectroscopy and Fourier transform spectroscopy. Electron paramagnetic resonance receives only very brief mention.

The principles of each type of spectrometry considered are very carefully set out, commencing with the background theory and leading on to the instrumentation necessary to make use of the general method. It is always difficult for an author to decide where to start. In this case the treatment is so elementary at the beginning of each section that one might think the book is intended for those who have almost no knowledge of physics and have not met even the laws of refraction nor the term "refractive index". On the other hand, a good facility in algebra and calculus is assumed. Such readers will be able to follow the book but will have mental indigestion before they have finished reading any one section. There must, however, be many who are experienced in some types of spectrometry—perhaps ultraviolet absorption and emission spectra—and who would like to consider the possibilities of some of the newer methods and they will find this book invaluable.

In general the book is written so that statements are very clear and precise.

Some of the criticisms on detailed points which one has noticed may be mentioned. The use of the phrase "continuum spectra" in place of the usual "continuous spectra" can be a little irritating. The author asserts that numerical wavelengths should always be the value in vacuum, whereas wavelengths in air at STP are normally used; the former is perhaps more logical although this means that the first two Balmer lines for hydrogen are given as 658 and 487 nm, instead of the usual 656 and 486 nm; but he is inconsistent and in Fig. 2.2 sodium wavelengths in air are indicated. A problem on page 42 asks for the quantum numbers of the outer electron of aluminium. This is not a fair question; from what he has been told, the reader will "guess" the right answer. But he will then have the impression that he can deduce the answers for the ground states of other atoms—which is certainly not true for the heavy atoms!

At the end of each chapter is a useful list for "further reading" together with an assortment of problems carefully designed to make the reader think. To summarize, any analytical chemist anxious to widen his knowledge of spectrometric methods will find it very useful to have this book on his shelf.

G. F. LOTHIAN

## Polymer Science

*Textbook of Polymer Science.* Second edition. By Fred W. Billmeyer, jun. Pp. xiv+598. (Wiley: New York and London, June 1971.) £7.50.

THIS textbook is a revised edition of that produced almost ten years ago. It comprises five sections dealing respectively with characterization of polymers, structure and properties, polymerization, properties of commercial polymers and polymer processing.

The characterization section serves as a brief introduction also, reminding the reader about intermolecular forces and bonds. Polymer solutions are dealt with in general, by stating the conclusions of theoretical studies and then applying them. This too is the case for measurement of molecular size and shape and for analysis and testing, this latter part devoted to physical methods.

The concepts of regularity and order are developed in discussing morphology, rheology and the relationship of physical properties to structure. Here again, the approach is readably descriptive, basic mathematical theory being referred to and results stated and used.

The section on polymerization deals with the chemistry, mechanism and kinetics and covers condensation, free radical addition, ionic, coordination and ring opening processes. This is extended to deal with the more complex situations which develop when

copolymerization systems are studied.

A useful part of the book is devoted to commercial polymers, where cost and performance have to be balanced and where minor variants of the main polymer may have important uses in speciality items. More technological details such as plasticization and stabilization are dealt with here, and the thermo-setting resins which do not fall easily into earlier more fundamental chapters.

The shorter final section deals descriptively with polymer processing and includes moulding, extruding, blowing and spinning, together with a brief mention of vulcanization.

To cover an extensive and advancing area of science and technology in detail in a single volume is clearly impossible, and in the present textbook the author has pruned away much of the detailed theory. The emphasis remaining is chiefly on a fairly descriptive level, showing what can be achieved by applying the results of theory to understanding complex polymer systems and relating these to the end technology.

This is a very readable book which can be recommended to student and practitioner alike, the former gaining a preliminary insight into this fascinating field of study and the latter having his horizons widened painlessly. The illustrations are good, many useful tables are provided, and the text is well referenced.

JAMES C. ROBB

## Fast Reactions

*Fast Reactions.* By David N. Hague. Pp. viii+159. (Wiley Interscience: London and New York, July 1971.) £3.

DURING the past twenty years the accumulated knowledge concerning rapid reactions, that is, those which have half-lives of less than a second, has grown enormously. So much so that "fast" reaction kinetics is now an established and indispensable part of chemistry and biology. Although there have been a great number of research papers and reviews published in this field, so far there have been few textbooks. The author states in the preface that this book was written as an attempt to bring to the attention of the general chemist or biologist some of the excitement experienced by those working on fast reactions, who are continuously developing and improving their techniques, thereby revealing interesting new information concerning chemical reactivity. Since fast reaction applications are so wide ranging, it is hardly surprising that in a book of this size the author seems to have been exceedingly selective in his attempts to convey an impression of significant advances. It is questionable whether his treatment which often only scratches the surface in one or two places is likely to achieve his declared aim.

The first half of the book is devoted to a fairly comprehensive description of the various experimental techniques which have been developed for studying fast reactions. A brief outline of flow methods is followed by a discussion of large perturbation methods including flash photolysis, pulse radiolysis and shock tube methods. A nanosecond laser photolysis system is described but there is no mention of picosecond lasers. Chemical relaxation methods which utilize a single small perturbing pulse are dealt with in some detail but the treatment of those based on the use of periodic disturbances is much briefer. The author's treatment of competition methods based on nuclear magnetic resonance, electron spin resonance, fluorescence and polarographic measurements which are covered in less than twenty pages requires a great deal of background knowledge which even industrious senior undergraduates are likely to find somewhat exacting. However, research workers in other areas of chemistry may find the concise treatment to their liking.

The second half of the book is devoted to selected examples of reactions which have been studied by rapid reaction techniques. In the third chapter proton transfer reactions and the reactions of the hydrated electron are discussed in some detail and several useful tables of elementary rate constants are given. This chapter concludes with a *résumé* of recent results and theories concerning metal complex formation. The final chapter gives some indication of advances concerning rapid reactions in biological systems with considerable emphasis on enzyme reactions together with comments on the role of metals and nucleic acids in biology. At the end of each chapter a short list of useful references is given; in addition there are some problems on the second and third chapters.

The book is well written and produced. One doubts, however, if it is likely to be preferred to previous textbooks which act as an introduction to the study of the kinetics of fast chemical reactions. In spite of this, most libraries will probably want to buy this concise and thoroughly readable book.

F. WILKINSON

## Inorganic Reactions

*Inorganic Reaction Mechanisms*. Vol. 1. Senior Reporter J. Burgess. Pp. xv+338. (The Chemical Society: London, June 1971.) £7.

SURVEYS of recent work are a valuable activity, of long standing, within the Chemical Society. The annual reports contained articles which, broadly speaking, fell into two classes. There were comprehensive reviews, covering all

work in a given area. Much rarer were occasional masterpieces in which selection and critical evaluation of recent trends were keynotes.

This inaugural specialist report on mechanisms of inorganic reactions is of the former kind. It contains a full though rather uncritical survey of relevant literature from January 1969 to August 1970, with no obvious omissions. There are extensive tabulations, particularly in the sections on oxidation.

Some errors have been missed by the proof readers. This is not too distressing because the production of these volumes is gratifyingly rapid. There are, however, several perpetuations of error, which are deplorable in a publication of the Chemical Society—nomenclature for optically active compounds is antiquated; the effect of acid on the aquation of halo-complexes is called, very inexactly, catalysis.

Many opportunities to resolve controversial points or to help generalizations to emerge have been burked. For example, the bare statement is made about  $[\text{Co}(\text{C}_2\text{O}_4)_3]^{3-}$  (page 200) that, "contrary to earlier suggestions that all twelve oxygen atoms exchange, a thorough investigation has shown that only six exchange with solvent oxygen". This implies that the earlier report is wrong. That work was done at a higher temperature, however, and, furthermore, the experimental graphs in the second report show that, even at the lower temperature used, slow exchange beyond six oxygens does occur. The two reports surely reflect the existence of two routes for exchange.

Apart from the last chapter on organometallic systems, which includes an interesting survey of new reactions, mechanism and kinetics (in spite of a disclaimer) are treated as synonymous. If we instead agreed with Nyholm that kinetics: mechanism=fact: fiction, and if we further accept that this year's science fiction may be next year's frontier of research, future volumes of this series might usefully redress the balance between kinetic and other aspects of mechanism.

R. D. GILLARD

## Artificial Intelligence

*Artificial Intelligence and Heuristic Programming*. Edited by N. V. Findler and Bernard Meltzer. Pp. 327. (Edinburgh University: Edinburgh, August 1971.) £6.

THE first NATO Advanced Study Institute on Artificial Intelligence and Heuristic Programming was held in Italy in August 1970. Fourteen leading artificial intelligence (AI) research workers were invited to lecture on the states of the art and science of AI to an audience consisting mostly of informed students. This book contains a set of papers, based on those lectures. A compre-

hensive index is included in the book.

Collectively, the papers embrace most of the major themes in current AI research—computational logic, problem solving and problem solving compilers, natural language processing and models of cognitive processing. The papers are well written and as a book cohere through the noticeable consistency with which individual authors have balanced the tutorial and research-report content in their papers, due no doubt, to the editorial effect of the audience in the summer school.

The paper by Robinson on building deduction machines, and related remarks in the paper by Raphael on the "frame problem", contain perhaps the clearest statements of these topical issues to appear in the literature. Robinson's paper ends with remarks about the need to find upper and lower bounds on the amount of work required to carry out specified tasks, a theme related to Meltzer's discussion of efficiency in proof procedures. Needless to say, we are a long way from being able to formalize efficiency in even the simplest of tasks. In fact, it is doubtful whether at present we can indicate how a calculus of efficiency (which necessarily must work over some procedure specification) might be formulated. Meltzer's contribution to this important issue embraces a framework for examining proof procedures which he uses to sharpen the relationships among incompleteness, completeness and heuristic. He gives a lucid account of proof procedures as composites of inference systems and search strategies and pinpoints the intrusions of appeals to human performance.

At the other end of the spectrum of concepts which motivate AI work, Kochen, in a wide ranging paper, discusses cognitive learning processes. He rejects the current trends in AI towards better heuristics, powerful deductive inference schemes and so on, as being an entirely inappropriate path to an understanding of cognitive learning. He proposes and argues for a model which allows him to order hierarchically what could be meant by knowledge, utility, comprehension and wisdom and within which specificity is lent to psychological terms such as understanding, awareness, intention, thought and so on. This rather broad philosophically oriented exposition is supported by a mathematical model and some suggestive implementation results.

The natural language area is covered by contributions from Lindsay, Simmons and Palme. In Lindsay's paper we find another extremely clear statement of an important AI concept—the influence of context on computation. Lindsay reports on the design of a natural language learning system based in part on this principle. Palme takes



a broader look at the field and includes a review which unfortunately loses a little through predating Winograd's work at the Massachusetts Institute of Technology. The paper contains a useful annotated bibliography. Conceptually allied to the semantically oriented approach to linguistic processing, Clowes's work on picture analysis invites us to consider the semantics of pictures. In his paper he argues cogently for the need to incorporate within a picture processing system the structure of what is depicted.

The development of heuristic search strategies and the means by which they can be influenced by specific problem contexts is an AI study area of considerable importance. Most AI programmes have search problems. In two complementary papers, Sandewall and Michie give a thorough discussion of the achievements in this area. Sandewall anatomizes the tree/graph type of search mechanisms, giving some well thought out terms for referring to various salient features. Michie is more concerned with the general problem of plan formation, which he presents from the standpoint of the heuristic search paradigm. He points out that only simple plans (that is, plans derived from fully specified input states and operator effects) can be made from existing heuristic search mechanisms alone.

Generalized problem solving techniques take their most convincing form when integrated into an acceptor for some language. This notion, together with the associated problem of designing declarative languages, leads to what are now called problem solving compilers. The book contains an informative paper by Elcock on this topic and a couple of related papers, one by Findler on the application of a new programming language, and one on a language for a game playing system by Pitrat.

Given that this book is essentially the proceedings of a symposium, it contains a larger than usual complement of high quality papers. With the treatment of the material leaning towards the tutorial, it will interest many "fringe" workers as well as the more obvious audience of AI students.

R. B. STANTON

## Polyatomic Molecules

*Rotation-Vibration of Polyatomic Molecules: Higher Order Energies and Frequencies of Spectral Transitions.* By G. Amat, H. H. Nielsen and G. Tarago. Pp. vi+441. (Marcel Dekker: New York, April 1971.) \$19.50; £9.30.

THE systematic study of the rotation-vibration spectra of polyatomic molecules dates from the classic 1936 paper of Wilson and Howard. The early work was brought to an initial level of com-

pleteness in a much used review article by Nielsen in 1951. However, it became apparent that higher-order contributions had to be considered to account quantitatively for observed spectra. For this reason a systematic treatment of the higher-order terms was undertaken in a series of papers begun by Goldsmith, Amat and Nielsen in 1956. The present volume is essentially a revised and unified presentation of this series, together with later additions from theses of students belonging to the Amat-Nielsen school.

Like the original papers, the book is devoted chiefly to the mechanics of the perturbation calculations, with little reference to actual spectroscopic observations or methods of interpreting them. The Darling-Dennison molecular Hamiltonian is introduced without derivation and is expanded as a power series. Two successive contact transformations are performed in order to eliminate as far as possible terms which are off-diagonal in the vibrational quantum numbers. This twice-transformed Hamiltonian is then in a convenient form for fitting to observed spectra. The notation required to keep track of the various contributions becomes quite elaborate, and numerous tables are given to relate the coefficients in the twice-transformed Hamiltonian to the basic molecular parameters. Extensive tables of matrix elements of the twice-transformed Hamiltonian are also given, with particular emphasis on molecules with three or four-fold axes of symmetry and on linear molecules. Most of the book (more than 300 pages out of 441) actually consists of tables either of matrix elements or of algebraic relations.

This monograph is obviously not designed for beginners. Workers interested in the finer details of microwave and infrared spectra may find it convenient to have this collection of results by a well known school brought together for ease of reference. However, I would have liked to see more consideration of the problems arising in the practical application of the formulae.

The book itself is reproduced by a photo offset process from a typed text, with most of the tables and special symbols drawn by hand. The result is perfectly clear, but seems excessively expensive.

J. K. G. WATSON

## Optical Methods

*Modern Optical Methods in Gas Dynamic Research.* Edited by D. S. Dosanjh. (Symposium held at Syracuse University, New York, May 1970.) Pp. xii+295. (Plenum: New York and London, 1971.) \$16.24.

THIS volume brings together sixteen papers contributed to a symposium on progress in gas dynamic research by

optical methods held early in 1970. They cover a wide and somewhat heterogeneous range of topics including shock tube spectroscopy and other radiometric methods in non-equilibrium flows; temperature errors in plasma diagnostics and their possible effects on absolute transition probabilities; impurity measurements in the expansion tube; diagnostics based on refractive index with particular stress on dispersion and hook interferometry; some optical diagnostic techniques involving lasers for the production of plasma, for holographic interferometry, for (Thomson) scattering; a summary of recent research on continuous wave chemical lasers; excitation by tunable lasers; and recent studies of the  $\text{CS}_2/\text{O}_2$  combustion laser.

In view of everyman's struggle to avoid being bowled over by the information explosion, it is unfortunate that the title of the volume appears to imply that it is a textbook or even a comprehensive review. There seems no good reason, other than sales promotion, for not having used the title of the symposium itself, though even prefacing the present title by "some" would have helped to increase the signal-to-noise ratio. As the proceedings of a symposium, the volume contains a very high proportion of valuable papers, as may be expected, since the contributions all seem to have been invited and the contributors include many distinguished names. As a collection of review papers purporting to cover the entire field, however, the book falls between two stools. It is too diversified and expensive to encourage the individual worker in any one of the disciplines covered to buy it, yet it is very far from being a comprehensive review. For example, in the field of refractive index diagnostics, there is no mention of perhaps 80 per cent of the new laser interferometric methods, the application of Schlieren methods is touched on without any reference to the problems arising in their use with laser sources, interference using Doppler-shifted light is not even mentioned, scattering measurements are concentrated around one particular regime, and so forth. As against this, no less than three papers deal with various aspects of dispersive (hook) interferometry. Presumably the coverage is due to the selection of contributors, and the form to the instructions issued to them. A useful comment on this is given by one of the contributors who concludes as follows: "The preceding text was written in a format primarily intended for oral presentation at the symposium, without specific citations. For the record, and the interested reader, some references weighted towards the author's work and without regard for completeness or priority are given. . . ."

None of this is a criticism of the individual papers, most of which are excellent and a few of which are indeed comprehensive reviews of the fields indicated by their particular titles. The contributions on chemical lasers and on hook interferometry perhaps deserve special mention. The comments concern only publication policy and, since this invariably succeeds, are a futile gesture anyway; if we cannot obtain relevant reprints from individual authors we shall just have to pay \$16.24 for the entire volume.

F. J. WEINBERG

## Aromaticity

*Aromaticity and Anti-aromaticity.* (International Symposium held at Jerusalem, April 1970.) Edited by Ernst D. Bergmann and Bernard Pullman. Pp. 398. (The Israel Academy of Science and Humanities: Jerusalem; Academic: New York and London, July 1971.) \$21.50; £10.05.

"WHERE have we anything to do with Mechanical Chymistrie that hath outstript the other Sects of Philosophy by her multiplied real experiences?" asked the Puritan, Noah Biggs, in his *Chymiotrophilos* of 1651, addressed to the Parliament of England. The present volume of quantum mechanical and physical organic chemistry devoted to the subject of aromaticity would doubtless have gratified Biggs, but he may well have felt that a subsidiary project in his grand design for the advancement of learning was left incomplete. "Where," he asked further, do we have "a Review of the old Experiments and Traditions, and casting out of the rubbish that hath pestered the Temple of Knowledge?"

The thirty-four contributions to the symposium held in Jerusalem in 1970 circle round the elusive concept of aromaticity with new calculations, new measurements and new syntheses, each attempting a closer definition. The new studies lead, however, to a fragmentation of the concept and generate overlapping sub-classes distinguished more perhaps by their prefix, pseudo-, anti-, quasi-, non-, homo-, and even pseudo-anti-aromatic, than by their content. As Heilbronner observes (page 22), many molecules might be termed "schizo-aromatic", for the allocation of a molecule to a sub-class depends on the particular chemical or physical property or theoretical measure used as a criterion. A possible exception may be the paradigm case of benzene, the arch-aromatic (Lloyd, page 89).

Historically, the concept of aromaticity has played a major role in the development of physical-organic and  $\pi$ -electron chemistry. Beginning as a loose description of compounds with an agreeable

aroma, "aromatic" became, with Kekulé (1865), an archetectonic term describing the molecular structure of six-membered ring compounds related to benzene. Classically molecular structures were proposed chiefly to account for chemical reactions, as opposed to physical properties, and aromatic substances came to be primarily characterized by their substitution reactivity (Robinson, 1925). Quantum chemistry introduced the new criterion of electronic structure in Hückel's rules (1931) and emphasis shifted to the electronically-dependent physical properties. Studies of these properties have undermined the old distinctions and introduced new ones which are far from concordant among themselves. The proceedings of the 1970 Jerusalem symposium suggest that the concept of aromaticity is a Wittgenstein ladder which chemists have climbed and now, on reaching the top, should throw away.

S. F. MASON

## Air Pollution

*Air Pollution Control.* Part 1. (Environmental Sciences and Technology: Wiley Series of Texts and Monographs.) Pp. ix + 451. (Wiley: New York and London, August 1971.) £9.50.

THIS is not a book on air pollution control, it is a set of seven disparate review articles on equally disparate subjects, bound up together. The reviews on the collection of particles by fibre filters, forty pages, and condensation effects in scrubbers, fifty-two pages, are both theoretical accounts with notes on experimental work to back up the theory, but little about practical applications. Both could be quite useful to a newcomer to research and development in these narrow fields, for the authors were allowed a reasonable space in which to expound their subjects and used it well. By way of contrast the author of the review on the dispersion of pollutants attempted valiantly to deal with this complicated and difficult topic in thirty-four pages. The rather breathless rush towards the end prevented adequate treatment of what promised to be the most valuable part of the chapter. In contrast again, the review on electrostatic precipitation occupies 110 pages on "updating prior surveys... with the emphasis on physical principles and predictive methods rather than on equipment and empirical descriptions". It ends, however, with a useful and wise, although all too short, discussion on applications, costs, difficulties and so on.

The chapter on the formation and control of oxides of nitrogen is a well balanced review of the role of these substances as air pollutants, as is also that on the control of internal combustion engines. This latter gives

essential practical details often lacking in some accounts of motor vehicle problems.

The remaining article, on the control of sulphur emission from combustion processes, is a very mixed bag indeed, and I was left with the uneasy thought of what the reader could conclude from it as to the present state of the art in these fields. While a great deal of accurate and useful information is presented it is mixed up with the pipe dreams of inventors of an earlier generation and no key to the maze is given. In some instances the information is inaccurate in fact, in others by implication. For example, it is stated that a Howden-ICI cyclic lime process was installed at Fulham power station in 1935 and a flow diagram and explanation of the method is given. It is not stated that the plant was shut down in 1939 and that the process is as dead as the proverbial dodo. The fact that, later in the chapter, the author copies out a detailed table of costs raises suspicions that he is not himself aware of the true position. The Fulham-Simon Carves ammonia process is described and it is stated that the North Wilford plant, commissioned in 1957, "handles the combustion waste gases from..."; it does not, as it was shut down some ten or eleven years ago. This sort of thing destroys confidence.

Looking back at the book as a whole, all the chapters contain excellent collections of references, and some of them consist, in effect, of useful sets of notes on these references. As such it has a certain real value, but a value that is very small indeed in relation to the price of £9.50 for the 451 modest sized pages. Perhaps for the first time I have understood my American colleagues who try to buy all their private technical books from British publishers in the United Kingdom.

S. R. CRAXFORD

## Time Past

*Geological Time.* By J. F. Kirkaldy. (Contemporary Science Paperbacks No. 46.) Pp. 133. (Oliver and Boyd: Edinburgh, April 1971.) 37½p.

BEGINNERS in geology often find difficulty in comprehending the enormous span of geological time. A million years is difficult to appreciate. But since an understanding of the duration and significance of time intervals is important in most branches of geology, the sooner that these figures are understood the better. What the author has done has been to explain in simple language why time is so important in geology and how it is measured.

The first chapter is a brief historical survey of ideas of the age of the Earth, showing how ecclesiastical dogma was dominant up to the nineteenth century.



Perhaps more significantly, however, it was the formulation of geological concepts, the laws of superposition and principle of uniformitarianism, which in the end led to ideas of duration of processes and hence to a relative time scale. Professor Kirkaldy stresses the importance of the early workers in these fields and goes on to demonstrate how the next step, of correlating strata by their included fossils, came about. Thus, with these fundamental principles, workers were able to erect stratigraphic successions and, in doing so, make relative datings of beds. The added complications of breaks and modifications (for example, unconformities and orogenies) in the succession are explained and their significance in working out a time sequence is demonstrated.

Following on from this, Professor Kirkaldy surveys the methods by which estimates of elapsed time have been drawn up. Thickness of sediments and rates of sedimentation as methods, salinity increases, palaeontology and evolutionary rates, and the rate of cooling of the Earth are discussed at length. All give markedly different ages to the same rocks.

After this survey of relative methods of age determination the book continues with an introduction to dating using radioactive isotopes. This is an exceedingly useful chapter, the physical concepts behind the determination being clearly set out and each of the principal isotope ratio methods explained. The specific advantages of each method are given together with the rocks, or more properly minerals, on which it is best used.

In 1913 Arthur Homes was one of the first to draw up an absolute time scale from data available. Scales were modified as new dates became available and as more precise techniques for isotope measurement were invented. In this part of the book the more problematical determinations of Pleistocene and Recent dates are examined and the reliability of dates throughout the stratigraphic column is pointed out. Methods of determining time sequences in metamorphic and highly folded and faults areas are indicated, and this has a bearing on the account of Precambrian time which follows. The book ends with a discussion of the age of the Earth and the solar system, and the significance that Moon dates have on this.

This is certainly a book which all beginners in geology should read, and it will provide a valuable refresher course to their more experienced colleagues. If isotope measurements are to be used it is important that geologists understand how they are calculated and how reliable the dates derived are, unless he is to accept them blindly.

J. R. V. BROOKS

## Soil Analysis

*A Textbook of Soil Chemical Analysis.* By P. R. Hesse. Pp. xxiii + 520. (John Murray: London, April 1971.) £7.50.

THIS book is designed to provide a course on soil chemical analysis and will prove to be a valuable text both for teachers and students. Practising soil chemists should also find much of interest in it. The two introductory chapters, covering the purpose of soil analysis, precision and accuracy, preparation of samples and the interpretation of results, are excellent; the author's critical awareness of the problems involved should help to dispel "the myth of soil analysis being the answer to the farmer's problem". The remaining sixteen chapters are given to a particular element, group of elements, or property of soil, and cover most chemical topics of interest to a soil scientist. The chapter on waterlogged soils is an innovation that should stimulate work on an important but neglected subject.

Most chapters have a short introduction, rather more extensive for lesser known topics, then a section on the background and theory of the determination, and finally practical details for recommended methods. A number of different approaches are described for most problems, covering the literature fairly thoroughly, and making the book a more comprehensive guide than has been available hitherto.

The analytical methods for which detailed instructions are given use chiefly simple apparatus so that they are well within the scope of the average soil laboratory; this should particularly commend the book for situations where resources are limited. Sophisticated instrumental methods are mentioned where they can make a contribution, but the reader is referred to other publications for details. Simple diagrams of apparatus, and flow sheets for some of the complex determinations, are welcome aids to clarity. Appendices contain references, bibliography and useful information on standard solutions, conversion factors and so on, and there is a full subject index.

Analysis of soil is a very different proposition from conventional chemical analysis, for the total amount of an element or compound is only rarely required. The need, rather, is for the proportion that is likely to be involved in the process under study, be it pedogenesis or plant nutrition. An essential prerequisite for effective procedures is thus an understanding of basic soil chemistry, and it is gratifying that one of the prime aims of this text is "to narrow the gap between the theory of soil chemistry and the manuals of soil analysis". That this gap still seems to be rather wide in some cases is due not only to the complexity of

soil chemistry, but also to the uneven treatment given here to the underlying theory, particularly the more physico-chemical aspects. There are also a number of misleading statements that undermine the book's authority on general soil chemistry. The author is clearly on safer ground when dealing with analytical procedures.

Rapid advances in this important practical subject will ensure that a second edition is required eventually. This should provide the opportunity to remedy some blemishes in an otherwise excellent publication. B. W. BACHE

## Analysis of Sediments

*Procedures in Sedimentary Petrology.* Edited by Robert E. Carver. Pp. xiii + 653. (Wiley: New York and London, June 1971.) £11.75.

THIS book is intended as a guide to the standard procedures for measurement and analysis of sediments. There are six principal sections which deal with the analysis of sedimentary structures—size, grain, texture, mineralogy and chemistry. Several authors contribute chapters to these sections and the coverage of subject matter is fairly complete. There are chapters on thin section making, heavy mineral separation, staining, point counting, peels and impressions, X-ray photography, X-ray diffraction, porosity and so forth.

Size analysis is well covered with a useful table of sieve data, sieve analysis, pipette analysis and related methods of sedimentation analysis (including the rapid sediment analyser using a water pressure transducer). Instrumentation for electronic grain size measurements in thin sections is described and a mathematical treatment of size distribution is followed by an examination of grain size shapes and grain surface textures. Scanning electron microscopy is used to identify the effects of littoral, aeolian and glacial environments on the surfaces of grains. The sections on mineralogical analyses give a good account of gravity separation, but magnetic and electromagnetic methods are not adequately described, nor is any reference made to the "micropanner" which is often a useful tool.

The chapters on X-ray photography are particularly interesting and more use should be made of it in petrology. The chief difficulty is the general lack of suitable medical/industrial-type X-ray units in most geological departments, although excellent X-radiographs of rocks can be obtained from a small apparatus employing a  $^{125}\text{I}$  source (Weiss, *Amer. Assoc. Petrol. Geol.*, **50**, 1507: 1966). There is a thorough treatment of optimum sample thickness and other parameters, together with descriptions of the more special techniques of stereo X-radiography which is of considerable value in studies of three-

dimensional texture and of fluid movement in sediments.

The section on chemical analysis might be misleading because it only deals with the determination of carbon (as total organic carbon) and carbonate-carbon, followed by a chapter on the measurement of Eh and pH. The techniques suggested for determining organic car-

bon are standard ones, including wet and dry combustion methods, the results of which are compared. Dry combustions were made on commercially built Coleman and Leco C-H analysers. In my experience, analyses of organic carbon in carbonate-rich sediments tend to be erratic when they are calculated by difference between

separate total carbon and carbonate-carbon analyses. The single sample method (Shaw, *J. Soil Sci.*, **10**, 323; 1959) is usually more reliable.

The book constitutes a valuable laboratory guide and source-book on the methodology of sedimentary petrology and has assembled a lot of widely dispersed literature. T. W. BLOXAM

## Biological Sciences

### Biodevelopment

*Molecular-Genetic Mechanisms of Development.* By Zhores A. Medvedev. Translated from the Russian by Basil Haigh. Pp. xiv+418. (Plenum: New York and London, 1970.)

It is part of the current biological faith that the advances in genetics and molecular biology will provide the means to understand embryology. Some molecular biologists are turning to developmental biology, and this is only right, for developmental biology occupies a key position in biology: it can be seen as the means whereby genetic information is expressed in terms of functional cellular patterns and organized forms. It provides the link between genetics on the one hand, and morphology and physiology on the other. What is not at all clear is the level at which rapid progress is to be made and I remain to be persuaded that, at this stage, studies at the molecular level will always be the most productive. While it might be true for the control of gene action and protein synthesis, a contrary argument might be made for such aspects of development as pattern formation, where the cellular basis is not understood. And even for gene action it seems that some of the striking recent advances have come from cell biology, namely nuclear transplantation and cell hybridization.

The book does not live up to its exciting title and, however good a molecular geneticist the author may be, he gives little indication of having insight into the problems of development. For him it seems it is nothing more than gene activation and that this is best understood in terms of thinking from bacterial models. Most of the book is concerned with molecular biology and biochemistry. It starts with DNA and protein synthesis and then goes on to virus reproduction. The emphasis here and elsewhere is on molecular genetics and, for example, while virus assembly is seen as a morphogenic process it is not treated as such. Self-assembly and its associated problems along Caspar-Klug lines is barely mentioned. This is followed by an account of induction in bacteria and chromosome replication.

Material more relevant to development is considered in the chapters on chromosome structure and function, and changes in proteins and nucleic acids during development. While they contain a great deal of data, they form rather unsatisfactory reviews because of the lack of an adequate conceptual framework. Biochemical morphology, such as changes in enzyme activity of four enzymes during development, is as dull as the duller traditional morphology. Also the style, perhaps due to translation, makes reading difficult: for example, "The issuing of programming instructions by the nucleus cannot be turned simply in time or in space (as in a certain type of symmetry), but it must conform to the actual morphological and physiological pattern of development, to the new conditions arising on account of the new relationships". The last chapters on the control of gene action are the most interesting but, because they were written in 1968, are somewhat out of date. Repetitive DNA, molecular hybridization, cell hybridization, recent work on nuclear transplantation are virtually absent and only receive brief mention in the last chapter written in an attempt to bring the book up to date. Cellular interactions, cell movement, polarity, determination, transdetermination, quantal mitoses, which are frontier regions of developmental biology, are just absent. This is classical molecular biology crudely applied to embryology. Perhaps the author, who is scholarly and very knowledgeable, is just too out of touch with current trends in the field.

LEWIS WOLPERT

### Cells and Molecules

*Advances in Cell and Molecular Biology.* Edited by E. J. DuPraw. Pp. xv+308. (Academic: New York and London, July 1971.) \$15; £7.

*Protein Synthesis.* Edited by Edwin H. McConkey. Pp. xi+298. (Marcel Dekker: New York and London, 1971.) \$19.50.

THE aim of each of these volumes is to provide reviews which will keep abreast

of research on the rapidly expanding topics of cell and molecular biology. Each promises to be just the first in a series, future volumes of which will appear at unspecified intervals.

What distinguishes these series from the many others already available? *Advances in Cell and Molecular Biology* is related to DuPraw's book, *Cell and Molecular Biology*, on which it will build. One of the hazards of writing books on topics such as these is that they tend to become out of date so very rapidly. By editing a series, to which contributions will be made by many authors, DuPraw intends to keep up to date the ideas discussed in his book.

But the contributions to the first volume of this series are written at a level very different from the basic text to which they are supposed to relate. Each article consists in part of a detailed review of the author's own work, in part of a review of research in general. The readers of the series of reviews will presumably be very different from those who would read the original text.

How useful will the series be as a collection of reviews of this type? I must confess that my prejudice is that series of this nature serve little useful purpose, except for those who are themselves working in closely related fields; but such people are usually fairly well informed in any case. What is needed so badly is a series of reviews which can easily be understood by those not themselves indulging in research on the particular topic to be discussed.

There seems, indeed, little to distinguish this new series from the many other review volumes with which cell and molecular biology is served. The emphasis of the first volume lies with the chromosome itself. Each review is very detailed, so that the uninitiated will find it difficult to follow the discussion. Each contains a number of references to be measured in hundreds rather than in tens; it will be a discerning reader indeed who can pick out the main lines of research from among them.

The aim of the series *Protein Synthesis* is to provide a critical evaluation of progress in research, both in eukaryotes and prokaryotes, and not merely to



list recent data. The contents of the first volume are concerned with the synthesis of the polypeptide chain, the structure and function of the ribosome, and the charging of tRNA. Most of the articles take a historical approach.

In spite of the restricted area discussed, the individual articles, although in the main well written, do not relate well to each other. But the two articles on protein synthesis, in particular, are brief and come close to achieving the aim of a critical evaluation of the state of play. The other articles are longer, some of them tending to take a more definitive stance instead of keeping strictly to one theme. Nevertheless, on the whole this volume is better written than most such reviews and readers would do well to keep an eye open for future volumes. BENJAMIN LEWIN

## Embryological Methods

*Methods in Mammalian Embryology.* Edited by Joseph C. Daniel jun. Pp. xv+532. (W. H. Freeman: San Francisco and Reading, July 1971.) £10.70.

MILESTONES in the development of mammalian embryology were the first sighting of mammalian eggs and pre-implantation embryos by Cruikshank in 1797 in the rabbit, and in 1890 Heape's successful transfer of fertilized rabbit eggs from one doe to another, which marked the initiation of experimental embryology in mammals. Fig. 1, based on the chronological distribution of references in Daniel's new *Methods in Mammalian Embryology*, illustrates graphically the steady rise in interest in the subject throughout the 1950s, leading to a formidable volume of work during the past decade. The decline in number of references since 1967 is an artefact: internal evidence suggests that most of the chapters were written before 1968, though some have been revised more recently.

It is intriguing to speculate why the subject took so long to get going, since few of the techniques involved require sophisticated modern equipment, and

most of the 1960s work could as well have been done in the 1920s. The most important single technical advance was probably the introduction of antibiotics during the Second World War. This democratized culture techniques of all kinds, and opened the way for *in vitro* culture and experimental manipulation of mammalian embryos on a large scale. Even more important, perhaps, was the question of social incentive. As advances in medical science led in many parts of the world to a rapid increase in population, the necessity of fertility control was belatedly recognized. At the same time the reduction in infantile mortality due to infections highlighted the significance of congenital malformations. Reproductive biology, including mammalian embryology, has in consequence become a well endowed and fashionable subject, no longer sequestered in some corner of a university zoology department.

*Methods in Mammalian Embryology* includes four chapters on eggs and spermatozoa, one on fertilization, eight on egg storage, culture and transfer, four on experimental manipulation, including micromanipulation, three on biochemical analysis, five on electron microscopy, autoradiography, histology, cytology and fluorescence techniques, four on organ and cell culture, and six on implantation and placentology. Although not entirely up to date, it gives a good all-round background, and each chapter is written by an expert who has used extensively, in many cases pioneered, the techniques that he describes. Tips range from the esoteric (Mintz's use of ant cocoons as mouse-egg containers for histology—"we have collected especially good quality sacs in sandy soil under stones and wood in New Jersey"; Blandau's impressive array of instrumentation for observing and photographing ovulation and egg transport *in vivo*) to the homely (Dickmann's demonstration that rat eggs can be transferred, if all else fails, in tap water; Enders's injunction to take care that flushings "land in the recipient container rather than on your lab coat").

There are some sections that could be drastically shortened, and some curious omissions. No mention is made of human embryos and the techniques such as amniocentesis, ultrasonic analysis, laparoscopy and foetoscopy, which are increasingly used in their study. Nothing is said about irradiation or about culture of pre-implantation embryos in any species other than the mouse. Particularly in the chapter on egg storage, there are many general statements, without reference to species, which may prove dangerously misleading.

The index is good. It seems deeply ungrateful to carp when editor and authors have in many ways done such an excellent job, but if the volume had

been half the size, half the price, and published two years earlier, it would have been more than twice as valuable.

A final word of warning. Although this book is a mine of useful information, it is no substitute for visiting a laboratory and seeing how things are actually done. ANNE McLAREN

## Regeneration

*Regeneration of Liver and Kidney.* (New England Journal of Medicine.) By Nancy L. R. Bucher and Ronald A. Malt. Pp. xiv+278 (Little Brown: Boston, Massachusetts; Churchill, Livingstone: Edinburgh, 1971.) £6.

IF a substantial portion of the liver of a rat or a mouse or a dog is surgically removed, the remaining fragment grows, rapidly at first and then more slowly, until it attains the approximate size of the original intact organ. Similarly if one kidney is removed, its partner grows to approximately twice its normal size. These parallel examples of compensatory growth or "regeneration" have been known for half a century, but the mechanisms involved are still very much a mystery. The *New England Journal of Medicine* recently published extensive reviews of liver regeneration by Dr Bucher and of kidney regeneration by Dr Malt. These have now been reorganized, expanded and brought up to date to form a short monograph. In the process the authors and their publisher have taken full advantage of the opportunities offered by publication in book form rather than in the cramped pages of a journal. The result is (like other volumes in the same series) attractive in format, typography and illustration. It contains a good deal of new material, and the old material seems to have been carefully revised. The style of the book is clear, concise and readable.

Though there is some difference of emphasis between the two parts of the book (kidney function being, for example, given more space than liver function), it is held together by the similarity in outlook of the two authors. Neither Dr Bucher nor Dr Malt has tried to provide a review of the sort which is a *catalogue raisonné* of everything which has been published in a given field. They have attempted instead to relate knowledge of regeneration in liver and kidney to the present understanding of protein and nucleic acid metabolism. Their introductory chapter is a summary, brief and austere but lucid and agreeably illustrated, of the relationship between chromosomes and protein synthesis. Of the 160-odd pages devoted to the liver problem, more than half are concerned with molecular biology in some way, and the same central theme is almost as dominant in the discussion of the

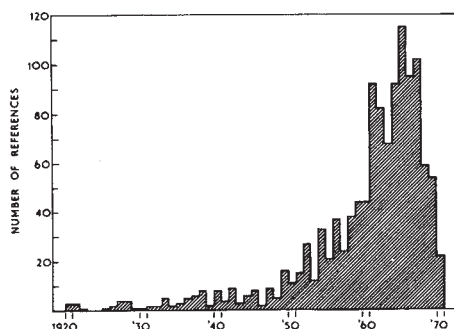


Fig. 1 Increasing interest in embryological methods, illustrated by the references given by Daniel in his book.

kidney. The success with which the authors sustain their chosen approach is remarkable. Attempts to interpret biological phenomena in biochemical terms are common enough but they are seldom supported by such a command of general biochemistry. Moreover, some of the difficulty and some of the attraction of liver and kidney regeneration as a subject for research is that it does not lie clearly within the province of any recognized discipline. It was originally opened up by experimental surgeons, but it has since engaged the attention of zoologists, physiologists, biochemists and pathologists. It is a considerable achievement to have organized material from so many diverse sources around a single central theme.

The one weakness in an otherwise excellent book is the brief comparison of regeneration in the two tissues with which it ends. Like everything which has preceded it, this is concise and lucid, but it could with advantage have been longer. The authors are leaders in their respective fields. Many of their experiments have been marked by novelty of approach and extraordinary technical virtuosity. Having devoted 250 pages to describing the existing state of knowledge they might have allowed themselves more than three pages to discuss their conclusions. R. Y. THOMSON

## After the Apple

*Gravity and the Organism.* Edited by S. A. Gordon and M. J. Cohen. Pp. ix+474. (University of Chicago: Chicago and London, September 1971.) £6.30.

ALTHOUGH this book covers a diverse and important field of knowledge it will be known to people who work in it as tightly compartmented. Thus, a vertebrate biologist will be familiar with the statoliths of fish and mammals, their mechanical responses to acceleratory forces including gravity and the associated neural mechanisms whereby these responses are translated into appropriate movements or sensation. But this is only part of a much larger picture. His ideas will need to be enlarged; for instance, by reference to the work of plant biologists on the geotropic mechanisms of plants, and in a variety of other directions. For this there is needed a freer diffusion of knowledge between the compartment walls and much is done to achieve this by the compendium of up to date information, well balanced and handsomely presented, which is to be found in this volume. This comprises the texts of thirty-nine papers, together with the discussions relating thereto, which were presented at a symposium on gravity and the organism, at Tuxedo, New York, in 1967. The contents have

since been updated to 1971, the year of publication.

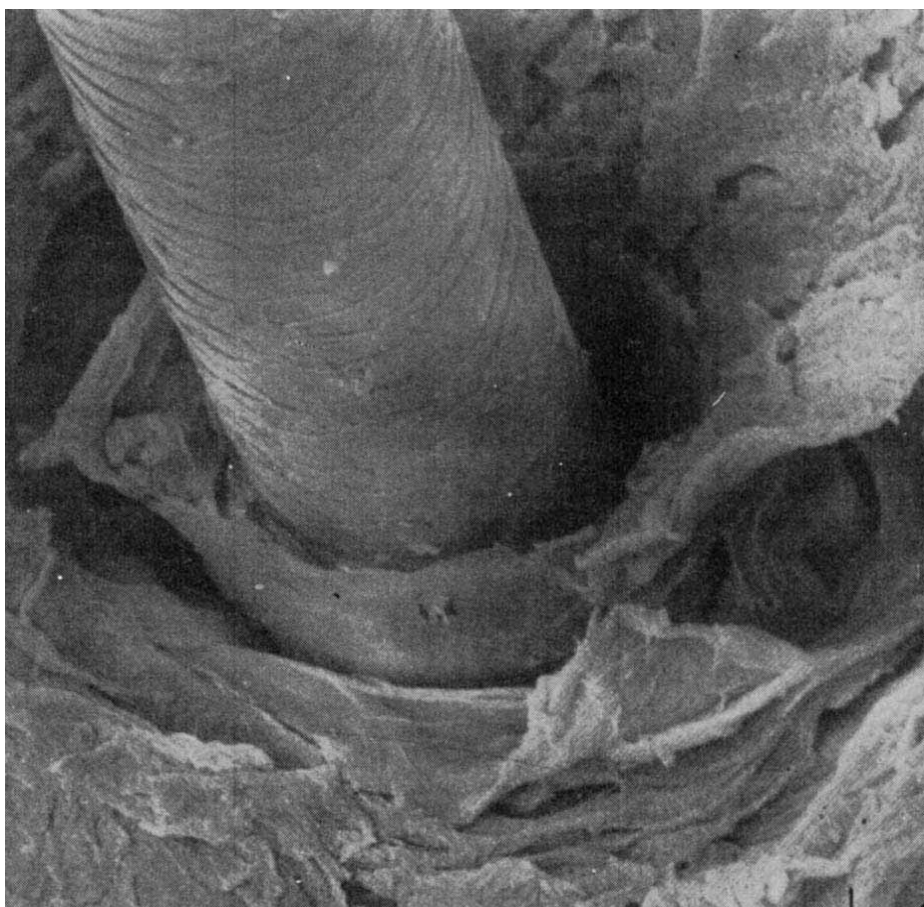
The meeting was sponsored by the Space Science Board of the US National Academy of Science with the support of the National Aeronautics and Space Administration (NASA) and attracted fifty-four participants. Twenty of the papers were from the United States and eighteen from European laboratories, including five from Great Britain. There was also a contribution from Japan. There is an excellent introduction by Dr A. H. Brown of the University of Pennsylvania, and summaries are provided by Professor O. Lowenstein of Birmingham and Professor A. W. Galston of Yale.

The resulting book is large; 470 pages are divided into seven parts, covering the reactions to gravity of plants (three parts), invertebrates (one part), and vertebrates (one part). There is also a large part (100 pages) devoted to gravimorphism in plants and animals. The final part contains an interesting summary of recent space studies.

The areas most familiar to me are

those concerned with the functional anatomy and gravity sensing mechanisms of the inner ear of animals. These are dealt with by Gualtierotti of Milan and Lowenstein. They are of notable excellence and include an up to date summary of electron microscopic data. Other sections of especial interest are concerned with gravity susception by plants. This, according to some workers, would appear to involve mechanisms essentially similar to those of the animal labyrinth. Here, use is made of statoconia which by virtue of their high specific gravity are displaced by gravity, stimulate the underlying sensory cells and so initiate movements of the animal which are appropriate to its gravitational circumstances. Larsen of Bergen presents very clear evidence that in plants the part of the statoconia is played by certain high density elements which are judiciously categorized as "sedimentary particulates". According to the positioning of the plant with respect to gravity these particulates, in some cases starch granules—amyloplasts—will vary their position

## Human Hair



The basal part of a hair of the human head. The epidermal surface is covered with leaf-like cells which, when peeled off, will form dandruff. (Magnification  $\times 1,020$ .) Taken from *Atlas of Scanning Electron Microscopy in Medicine*, by T. Fujita, J. Tokunaga and H. Inou. (Tokyo: Igaku Shoin; Amsterdam, London, and New York: Elsevier 1971.) £9.00.



within the plant's cells and by acting on their walls in some way determine the occurrence and direction of the plant's growth. In a later section Edwards and Gray of Atlanta, Georgia, give a valuable account of the intracellular changes observed to occur when plants are exposed to chronic acceleratory forces up to 150g. Some of the findings are considered to support the view that the amyloplast granules of plant cells act as gravity receptors.

In the absence of structurally definable nervous or receptor elements large gaps in the argument remain to be filled. Here the evidence relating to the action of the hormone auxin and other growth regulating substances is highly relevant and the contribution of Wilkins of Glasgow is of much interest.

It seems right to say that all who concern themselves with gravity and its effects on living organisms will owe much to the organizers of this symposium. The publication of its records in the form of this book is a notable achievement in the dissemination of scientific knowledge.

C. S. HALLPIKE

## Ions and Membranes

*Ion Transport and Membranes—A Biophysical Outline.* By A. B. Hope. Pp. 121. (Butterworth: London, March 1971.) £3.00.

THE content of this book is not a great deal more than often found in a review article. It consists of just over a hundred pages which make up ten short and concise chapters. Although the text is limited it does adequately cover the basic theoretical concepts and experimental approaches which are currently associated with membrane studies of both plant and animal tissue. In fact the comparison between the properties of plant and animal membranes is an unusual and particularly valuable feature of this book.

The first chapter is concerned with the derivation of well established equations describing the distribution and passage of ions across membranes, including the relationships derived from the general flux equation such as the Goldman expressions. The use of irreversible thermodynamics for theoretical analyses of interactions between membrane fluxes is the subject of the second chapter. Only an elementary introduction is given but it is sufficient to give the reader an insight into the possible applications of this branch of thermodynamics to understanding the nature of solute and water fluxes in biological systems.

In the following chapters the author has attempted, where possible, to use much of the theory given in the earlier part of the book for quantitative analyses of experimental observations

both with artificial and biological membranes. Two chapters discuss evidence for active ion transport processes in plant and animal cells and their dependence on cell metabolism. As well as outlining present biochemical knowledge of cation pumps in animal and plant tissue there is some discussion into the possible mechanism for light induced chloride fluxes in plant cells.

Other chapters briefly discuss the electrical characteristics of plant and animal membranes with reference to excitability and correlation of ionic fluxes with measured conductances. There is also a short but useful chapter on the properties of various types of artificial membranes.

Provided this book is used in conjunction with a reasonably detailed lecture course, I would thoroughly recommend it for specialized undergraduate and postgraduate studies. I also think that the author has produced a book which will be particularly valuable as a source of reference for plant and animal physiologists who have research interests in this field of cellular function. Considering the length and nature of this book it is a great pity that the publishers could not produce it in a paperback form at half the present price.

J. BARBER

## Histones Reviewed

*Histones and Nucleohistones.* Edited by D. M. P. Phillips. Pp. xii + 305. (Plenum: London and New York, 1971.) n.p.

THE editor of this recent addition to the very limited number of books devoted to the topic of histones and the enigma of their structure and function in the cell comes from a well known stable in the histone field, although individual chapters are written by authors from several countries. The balance between structure and function is well maintained.

After an introductory chapter on preparation and characterization by E. W. Johns, giving a comprehensive survey of methods with a good coverage of references, we are launched into comparisons of the primary structures of histones and protamines by D. M. P. Phillips. Although this is the smallest chapter it will receive a great deal of attention in view of the current search for an explanation for the dramatic lack of species variation in the primary structures of histones. The author develops the theme of gene duplication to account for homology among histones as has been proposed for the protamines, and leaves one waiting eagerly for further information to become available. One criticism of this chapter is the confusing use of both the older three letter abbreviations for

amino-acids and the more recent single letter code—we sometimes get one, sometimes the other and on occasion both, giving unnecessary duplication.

In the third chapter E. M. Bradbury and C. Crane-Robinson give an authoritative critical review of work on the conformation of histones and nucleohistones. This clearly indicates that ideas in this field have not gelled yet, and there is still scope for the brain-wave to explain the interrelations between histone and nucleic acid and for the emergence of new techniques.

Unfortunately the authors do not define what they mean by nucleohistone here, for the acidic proteins of the cell nucleus get no mention or part to play. This contrasts with the next chapter by E. Fredericq on the chemical and physical properties of nucleohistones where the acidic proteins present come in for frequent comment in an established portrayal of properties of nucleohistones, collected together for critical appraisal.

It is, however, chiefly for the last two chapters, which constitute a third of the volume, on the functional aspects of chromatin, that the book will be bought and read avidly.

Histone biosynthesis is dealt with by Hnilica, McClure and Spelsberg, first in general terms and then in relation to embryogenesis and the gametes. Very little is left out, and with an up to date list of 282 references the reader feels that he is getting the total picture, including relationships with non-histone proteins.

The final chapter on functional aspects of DNA-associated proteins by Vincent Allfrey provides a lucid summary with 384 references. There are unanswered questions on all sides but this is the nature of the beast and the questions that should be asked become more apparent as the chapter progresses.

A minor irritation in the book is the use of independent chapter references, which entails the happy game of finding the end of the chapter if ever you want to check a reference. But the book is without doubt better than any of the few previous books on histones, giving both a good introduction to the subject and the latest state of the art; it will not become out of date too quickly.

H. J. CRUFT

## Terpenoids and Steroids

*Terpenoids and Steroids.* Vol. 1. Senior Reporter K. H. Overton. Pp. xi + 557. (The Chemical Society: London, July 1971.) £11.

THE aim of this volume is to provide detailed coverage of the literature on terpenoids and steroids from September 1969 to August 1970. It is the first of the

Chemical Society's specialist reports in this field and the authors have therefore included, where necessary, material published in the preceding year. With such a wide range of natural products, within which there are no sharply defined boundaries, it is difficult to decide on the best framework for a report. The procedure adopted takes cognizance of structural relationships, and of the biogenetic paths which provide the basic unifying theme. Part one (terpenoids) thus contains chapters on mono-, sesqui-, di-, tri-, and poly-terpenoids: the last chapter deals with the biosynthesis of both terpenoids and steroids. Part two (steroids) consists of two long "chapters" (the first on properties and reactions, the second on steroid synthesis) which are suitably divided into well chosen smaller sections. The intrinsic importance (biological and pharmacological) of steroids, and their suitability as substrates for mechanistic and spectrometric investigations, is recognized by the almost equal lengths of the two parts of this volume.

The eight authors are to be congratulated on the breadth and depth of their reading. Although exercising discretion in the material reported, they seem to have included all the important developments. Moreover, they have done much more than merely providing a comprehensive list of up to date references for further study: within a reasonably sized volume they have been able to summarize the chief points and to discuss briefly the significance of the publications to which they refer. Even the most active follower of the literature could not have hoped to have kept abreast of so many topics; organic chemists working on any aspect of terpenoids or steroids will find in this admirable volume a most welcome opportunity to see what advances have been made recently in all the related branches of research.

G. D. MEAKINS

## Foreign Substances

*Foreign Compound Metabolism in Mammals.* Vol. 1. Senior Reporter D. E. Hathway. Pp. xvii + 455. (The Chemical Society: London, May 1971.) £11.

IN several countries of the world official bodies that formulate regulations concerning the safety in use of drugs, food additives, pesticides and the like now require information about what happens to these chemicals in the animal body, and in the past decade or so there has been a tremendous increase in the output of information on this subject. Much of the older work in this field was covered in the second edition of my book *Detoxication Mechanisms* which appeared in 1959; the volume under review covers the period 1960 to 1969.

This book is the first of a series planned to cover a variety of aspects of the metabolism of foreign compounds and future volumes are to include chapters on special techniques such as whole-body autoradiography and subjects such as drug tolerance and addiction. The six chapters are concerned with: (1), absorption, distribution and excretion; (2), the movement or transference of foreign compounds in the body; (3), the kinetics of drug metabolism; (4), biotransformations — an American term meaning the molecular reactions that compounds undergo in the body; (5), the mechanisms of biotransformations; and (6), species, strain and sex differences in metabolism.

The first three chapters aim to give an introduction to all aspects of the absorption, distribution and excretion of foreign compounds. Such subjects as the permeability of membranes, protein binding, the use of radioactive labels, the various physiological systems of the body and their involvement in drug metabolism, the kinetics of drug metabolism and drug interactions, to mention only a few, are discussed. These chapters are an up to date and readable account of these processes, and can be read with benefit by anybody in and intending to enter the field of biochemical pharmacology and toxicology because they form an important introductory background to the rest of the book. The fourth chapter is by far the largest in the book and covers more than 40 per cent of the pages. It gives a succinct account of the metabolic transformations of selected drugs, food additives, carcinogens, insecticides, herbicides, molluscicides and so forth. It is not claimed to be comprehensive because a coverage of the metabolism of all the compounds studied in the past ten years would require several volumes.

The fifth chapter is a systematic account of the mechanisms of the transformations of foreign compounds in body tissues covering the processes of oxidation, reduction, hydrolysis and conjugation and their enzymatic intricacies. The last chapter deals with three of the many factors which affect the metabolism of foreign compounds; namely, species, strain and sex. Species and strain are factors of considerable importance in the problem of extrapolating results from experimental animals to man. Sex differences in drug metabolism are not important except in rats which, nevertheless, are extensively used in testing.

This book is a useful addition to the literature on the fate of foreign compounds in animals, but unfortunately it does not possess a compound and subject index. The metabolic fates of many chemicals are competently described but to locate a particular compound is tedious.

R. T. WILLIAMS

## Intermediary Metabolism

*Regulation of Gluconeogenesis.* (Ninth Conference of the Gesellschaft für Biologische Chemie.) Edited by Hans-Dieter Söling and Willms Berend. Pp. xii + 347. (Georg Thieme: Stuttgart; Academic: New York and London, July 1971.) \$12.50; £5.85.

THIS volume is the published result of a similarly entitled conference held in Göttingen in the spring of 1970. As such, it consists of twenty-five invited contributions together with full reports of the informal discussions which followed each principal paper. Even at a first glance it is clear that this book represents something of a turning point in the development of ideas concerning the loci of the rate-limiting steps of gluconeogenesis. To give a single example, it is instructive to note that both glucose 6-phosphatase and fructose 1,6-diphosphatase are conspicuous by their absence, both in the formal and the informal presentations. Even glyceraldehyde 3-phosphate dehydrogenase, a one-time favourite with some workers, has now been forsaken as a possible site of physiological regulation, and there is in evidence a general, although not unanimous, closing of the ranks behind the hypothesis that phosphoenolpyruvate carboxykinase, theoretically always the most likely candidate, is the site of gluconeogenic control. Indeed, much of the text is both directly and indirectly devoted to the consideration of the physiological role of this little understood enzyme and of its neighbour, pyruvate carboxylase.

In any new journal of inconclusive and negative results, the output from gluconeogenesis research groups would perhaps loom largest among the total number of contributions. One must therefore be particularly grateful to the editors and publishers of this volume for the provision of such full coverage of the informal discussion which contains much comment and material that would not normally be considered acceptable for publication by the established scientific journals. Among the problems dissected are: (1), compartmentation phenomena, both intercellular and intracellular; (2), the relationships between pyruvate carboxylase and phosphoenolpyruvate carboxykinase *in vivo* and *in vitro*; (3), gluconeogenesis, or, rather, the lack thereof, in homogenates; (4), the relationships between experimental and physiological situations, particularly with regard to hormone concentrations; (5), the interpretation of metabolite concentrations and crossover data; and (6), the role of the ubiquitous adenosine 3'5'-cyclic monophosphate.

On the debit side, one might perhaps have hoped for some information on, for example, ruminant gluconeogenesis and the role of energetically wasteful



cycling; it is also regrettable that the book contains so many not altogether trivial typographical errors and has no index. With these very minor reservations, however, this volume will be warmly welcomed by everybody interested in carbohydrate biochemistry, and not least by final year undergraduate and postgraduate students who may thereby gain some insight into the present problems of intermediary metabolism.

C. I. POGSON

## Dog's Life

*Behaviour of Wolves, Dogs and Related Canids.* By M. W. Fox. Pp. 214. (Jonathan Cape: London, September 1971.) £3.60.

A BOOK of this title is likely to arouse interest among students of animal behaviour and pet-keepers alike; it is probable, however, that the first of these two groups will be disappointed by this volume.

It touches on many problems of social behaviour and organization, and of the evolution of behaviour, and discusses some of the consequences of domestication, all in an easy flowing style. But too often the author is carried away by his writing, and the book leaves the impression of a series of arguments put together in a somewhat haphazard fashion, with many non-sequiturs. For the dog-enthusiast, however, these objections are to some extent compensated for by the presentation of several interesting observations, by the nice illustrations and the excellent layout.

The first chapter, "Evolution and Classification of Canids", provides a detailed discussion on the classification within this family, but little on evolution of behaviour.

In the next chapter the occurrence of various categories of behaviour in different canids is discussed. The comparison of the facial expressions of these carnivores with those of primates is of special interest here, and this is considered in detail. I missed, however, an evaluation of the function of behavioural differences between the canids from an evolutionary point of view. This is indicative of a general shortcoming of the book, which is the lack of reference to ecological studies and behavioural field observations. This shortcoming is irritating especially where the author writes, for example, about the "gradation of sociability from the solitary fox-like species to the more dog-like species". The fact that one can keep several individuals (for example, *Otocyon*) together in a cage does not signify that they are gregarious in their natural environment (*Otocyon* lives in pairs).

The seventh and eighth chapters are probably the best sections of the book,

and although they lack any quantitative information, they are interesting because of the author's many years experience in rearing wolves. They deal with development of social relationships and wolves as parents, and it is here that a good background will be found by those interested in the behaviour of domestic dogs. Again, there are fine illustrations.

The tenth chapter, on the comparative ethology of the domestic dog, states differences and similarities between domestic dogs and several other canids, and considers the effects of domestication. There is a long discussion of scent marking and territory, in which the author states "... territorial marking, through which one pack (of wolves) or one individual (a red fox) acquires information as to the population density of like-species in a given region". It is one of the many places where the author makes bold statements on a subject which has been extensively discussed by others previously, in a fashion which makes one doubt whether he gave it a great deal of thought.

It would have been useful to have an index, and I was sorry to see the excellent presentation of this book marred by spelling mistakes in almost all the German titles of the list of references. On the whole it is rather an unsatisfactory book to the behaviourist, yet certainly worth the effort of borrowing it from a library.

H. KRUK

## Go to the Ant...

*William Morton Wheeler, Biologist.* By M. A. Evans and H. E. Evans. Pp. xii + 363. (Harvard University: Cambridge, Massachusetts; Oxford University: London, July 1971.) £5.25.

THE biography of a scientist who died nearly 35 years ago might either be deemed to be too late to excite interest, or too soon advantageously to rekindle it, but the large number of Wheeler's old students will find it a delight. Others like myself who were influenced directly only by Wheeler's writings will be glad to learn more of the makings of this man, born in 1865 in Milwaukee, whose working life spanned so many of the formative years of American university development and of American science.

Wheeler's early education was at the German-English Academy in Milwaukee, where there flourished a highly cultured community largely of immigrant German stock. He left with a lifelong interest in the classics and in music, a fluency in the German language and a passionate interest in natural history.

When Milwaukee staged an industrial exposition in 1883, an especially attractive exhibit came from Ward's Natural Science Establishment in Rochester, New York. This included a large collection of animals, fossils and casts of fossils. Young Wheeler, still

## Animals of the Arctic



Seven species of phocid seal are found in the Arctic, though their patterns of distribution in the region differ widely. They and all other species of Arctic wildlife are documented and illustrated in *Animals of the Arctic: The Ecology of the Far North* by Bernard Stonehouse. Pp. 172. (Holt, Rinehart and Winston, New York, November 1971.) \$10.95.

at the academy, helped Ward set up the exhibits. Soon afterwards he took his first permanent job with Ward in Rochester, where he published a *Catalogue of Specimens of Mollusca and Brachiopoda for Sale at Ward's*, Ward supplying biological material to various universities, schools, museums and private collectors.

Milwaukee decided to found a permanent museum with Ward's special exhibit as a basis and Wheeler returned home to work there, and soon became director. In this new museum his friend and colleague Carl Akeley created the first museum habitat group—a scene of muskrats in a pond. Akeley's later work became a feature in the Field Museum, Chicago, and of the American Museum of Natural History, New York, which set the trend towards realism now followed by many museums.

In the summer of 1889, Wheeler visited the new laboratory at Woods Hole, directed by his former teacher at the German-English Academy in Milwaukee, C. O. Whitman. In this stimulating environment he met biologists such as T. H. Morgan, J. Loeb, E. G. Conklin and H. F. Osborn and many others, and developed a love for marine animals. In 1890 he followed C. O. Whitman to the newly founded Clark University in Worcester (Massachusetts) where he obtained his doctorate for a thesis on the embryology of insects in which he described in detail the embryonic writhings — blastokinesis — which are peculiar to insects with yolky eggs.

By 1892 he was teaching in Chicago and from there he visited Europe, spending some months with Boveri in Würzburg where he used the latest methods of section cutting to study the excretory system of the lamprey. He continued his summer visits to Woods Hole and in 1894 published a detailed study of the curious sex change in the marine worm *Myzostoma*. It was his move to Austin, Texas, in 1899 which brought him into contact with the ants which he studied with such fascination in later years. In 1908 he became professor of economic entomology and later dean at the Bussey Institution in Harvard. These years at the Bussey were the most influential in his life and he stayed there until his retirement in 1934, to die three years later.

Wheeler's teaching at so many new institutions had considerable influence on the development of biology. His books, *Ants* (1910) and *The Social Insects* (1928), were widely read by biologists. These books emphasized that what the animal really did was at least as important as its structure alone; so was born the subject which he termed ethology which has more recently been developed so successfully by Lorenz, Tinbergen and others.

A collection of essays, *Foibles of In-*

*sects and Men* (1928), in which his barbed shafts of wit, "liberally suffused with formic acid", aimed largely at "the stuffy and the solemn, not excluding professors of biology", reached the widest circle of readers. Wheeler himself did not rate the value of his scientific contributions in relation to their popularity; he took the greatest pride in his taxonomic work on various groups of flies and on the ants.

Readers of this book will ponder on the contrast between Wheeler's time and ours. Who nowadays could change his job so frequently and work in so many institutions and make such long and profitable trips abroad? Who could now produce important publications in so many biological fields?

Wheeler himself had the gift of clear exposition. His biographers share this ability so that the book can be read, or dipped into, with interest and enjoyment. The book includes twenty-one illustrations of insects and men, a list of Wheeler's 680 publications and an index which will greatly help readers to piece together the strands which make up this book.

G. C. VARLEY

## Below the Sea Bed

*200,000,000 Years Beneath the Sea.* By Peter Briggs. Pp. 228. (Holt, Rinehart and Winston: New York, Chicago and San Francisco, September 1971.) \$7.95.

THE author of this book has considerable experience at sea and as a science journalist. The title of his book refers to the oldest sediments recovered by coring deep holes drilled into the ocean floor during the past three years by the very advanced US exploration vessel *Ocean Challenger*. The book tells chiefly about the ship and the work at sea but the author sets his story against a background of all the evidence and ideas that have led to the new theory of seafloor spreading. It describes how the theory has evolved from topographic, seismic and magnetic studies of the deep ocean floor, particularly of the mid-ocean ridges and their rift valleys in which the continuing upwelling of basalt forms the new ocean floor, and continues with the concept of moving plates of seafloor and continental crustal material now used to explain the great mountain ranges, deep sea trenches, vulcanism and earthquakes. Where they collide, one sinks below the other and where ocean floor moves towards the land as along the west coast of America, seafloor material is sub-thrusted below the land. Some of the metalliferous provinces and ore concentrations are being explained in these terms, and from the practical as well as scientific point of view study of the sediments and rocks below the sea bed has become exciting and important.

After a general introduction to the history and background geology, the author tells of the now almost legendary American Miscellaneous Society and its drilling committee whose informal and formal discussions and negotiations led to the Mohole Project and to experimental drilling in deep water. The author implies that the successful investigation of the sediments was one of the factors which diverted interest from the more spectacular project of drilling right through the Earth's crust. It established the feasibility of the Deep Sea Drilling Project that followed, and provided much of the necessary design data. The new project is managed by the Scripps Institution, but the ship is run by Global Marine Inc, a sizable commercial oil-drilling concern which achieves real economy by choosing good men and paying and treating them exceptionally well. They work hard and are unquestionably competent and interested in new methods and equipment. The whole ship's company, about seventy in all, is changed at the end of each voyage of about two months and the officers and crew alternate one voyage on and the next on vacation. The decisions are made promptly by the specialists directly concerned, and who best understand the scientists, sailors and work at sea. There is none of the over centralized regulation, and insistence on controlling everything of many government funded projects.

The book describes the day to day successes and difficulties of thirteen voyages, equivalent to well over two trips round the world. The reader gets an impression of about two hundred deep drillings, the longest drill string being more than 4 miles and the deepest sediment penetration 3,231 feet. Drilling was sometimes stopped by a hard layer of flinty rock called chert which wears out the diamond bits after a few feet of drilling, but a method of re-entering the same hole has been worked out and tried successfully at a depth of 2 miles. A bit-changing operation might take 15 hours. More than three hundred US scientists have been concerned in the project and perhaps a tenth of this number have been from abroad. The cost for 4½ years work is to be nearly 35 million dollars.

The author gives a lively impression of how the programmes are planned and executed. He emphasizes the value of invertebrate palaeontology which seemed a very remote sort of science before the days of oil wells, and which now provides key personnel for the work at sea. Geologists and scientists who have worked at sea will probably be the most appreciative and satisfied readers of the book but it can be recommended to anybody with scientific interests.

G. E. R. DEACON



(Continued from page 267)

time and more efficiently lost from the surface. Such a decoupling of the short lived and long lived radioactivities at 4.5 billion yr ago from the outer 200 km of the Moon might be expected to have considerable effect on the Moon's subsequent history. Because of the localized concentrations of  $^{40}\text{K}$  in the fescon, local pockets of basaltic liquids associated with fescons would be formed. Impact-triggered eruptions could result in flooding of the regions above the fescons (Fig. 3) with spillage into adjacent areas. The mascons themselves may be primarily a result of near-surface effects associated with mare formation<sup>14,15</sup>, but the distribution of circular maria on the lunar surface and the production of basaltic liquids a billion years after the origin of the Moon result from the formation of fescons.

Many workers have noted that the mare basalts show gross depletions of the alkalis, and several chalcophile and siderophile elements relative to chondrites. The depletion of alkali elements has been attributed to volatilization from the Moon<sup>14,21</sup> and to pre-lunar fractionation processes<sup>22,24</sup>. Evidence obtained from Pb-U and Rb-Sr isotopic techniques<sup>4,25</sup> shows that these fractionations have occurred before or during the first few million years of the Moon's history. Strong arguments against volatilization following lunar accretion as an exclusive source of these fractionations have been presented<sup>22</sup>. In our model, the Fe-S liquid segregation will cause a depletion of the chalcophile, siderophile, and alkali elements, in addition to any pre-lunar fractionations.

Following the initial melting 4.6 billion yr ago, the outer regions of the Moon underwent extensive fractional crystallization<sup>9</sup>, producing the anorthositic crust by flotation of plagioclase cumulates<sup>15</sup> accompanied by sinking of pyroxene and ilmenite. The net result of this fractional crystallization process would be a mineralogically and chemically zoned outer region of the Moon, grading from Fe, Ti rich material in the lower parts to residual material near the top enriched in trace elements such as K, Ba, rare-earths, U and Th, overlain by an anorthositic crust.

About 1.0–1.5 billion yr later, the heat generated in the fescons by  $^{40}\text{K}$  would partially melt the already chemically differentiated outer layer, producing the ferro-basalts of the maria. Thus, the abundances of many trace elements such as Ba, U, Th and the rare earths in these mare basalts would have been controlled both by the enrichments in fractional crystallization in the initial melting and the partial melting about a billion years later. The alkalis, in contrast, would have been depleted in the initial melting because of the Fe-S liquid segregation, but would be somewhat enriched in the later partial melting.

Assuming a uniform thermal gradient from 200 km depth to the surface and a conductivity of  $10^{-2}$  calories/cm s °C, approximate calculations show that to produce and maintain silicate melting temperatures ( $\sim 1,000^\circ\text{C}$ ) (at the depth of the Fe-S layer) during the first billion years would require the extraction of  $\sim 100$  p.p.m. of K from the overlying silicate material into

the Fe-S layer. A 5% FeS content in the Moon initially would lead to a K concentration of 0.2% in the Fe-S layer. Estimating the K content of the source regions of the mare basalts at  $\sim 200$  p.p.m.<sup>26</sup> we obtain an initial abundance of 300 p.p.m. K in the outer parts of the Moon, significantly less than that in ordinary chondrites. An entire Moon of this K content would not undergo melting in the interior regions, as Urey and MacDonald showed<sup>27</sup>.

The hypothesis of Fe-S segregation and formation of fescons used in our model of early lunar differentiation can be tested in future lunar missions. Seismic studies bearing on the interior structure of the Moon should in principle detect the Fe-S layer and the fescons. The localized concentration of  $^{40}\text{K}$  in the fescons may be reflected by higher heat flow in the circular mare areas and can be verified by heat flow studies.

We suggest that Fe-S segregation plays a major role in the early chemical and physical history not only of the Moon and Earth<sup>20</sup> but of other terrestrial planets as well. Thermal histories of the planets attendant on a possible early segregation of K from U and Th remain to be studied.

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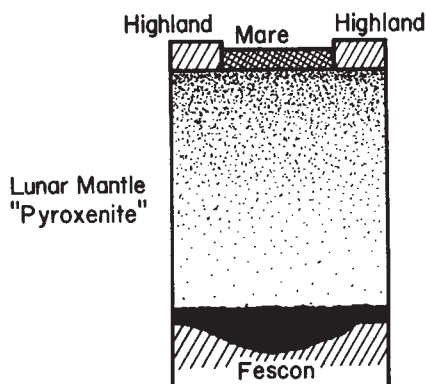


Fig. 3 Fescon and mare 3.0–3.6  $\times 10^9$  yr ago.

## New Prospect for Gamma-Ray-Line Astronomy

THE prospects for gamma-ray-line astronomy associated with the anticipated occurrence of radioactivity in interstellar space have already been outlined<sup>1-4</sup>. Each successive clarification of these possibilities has resulted from major clarifications in the theory of nucleosynthesis, and in this communication I wish to report another step. The supposition that the elements are coproduced in exploding stars has been greatly strengthened by quantitative success<sup>5</sup> in the past few years. A new development within that general picture now suggests that detectable amounts of <sup>60</sup>Fe exist in nearby explosive remnants. <sup>60</sup>Fe has a half-life  $\tau_{1/2} = 3 \times 10^5$  yr, and the subsequent decay of its <sup>60</sup>Co daughter is accompanied by a two-gamma-ray cascade with energies  $E_\gamma = 1.17$  MeV and 1.33 MeV (ref. 6). This pair of equal-intensity lines is the signature that can identify <sup>60</sup>Fe in interstellar space. The other important feature is, of course, its half-life, which is long enough to allow the radioactivity to persist long after (in terms of recorded history) the explosive event.

For discussion I take this very simple model: all solar nuclei were coproduced in  $1.7 \times 10^9$  explosive events occurring on the average every 3 yr over the first half of the galactic lifetime and now occurring once every 30 yr<sup>7</sup>. This model lowers the present supernova yield by peaking nucleosynthesis early. An elementary calculation shows that each event on the average must yield  $0.06 M_\odot$  of <sup>24</sup>Mg from the carbon shells to account for the roughly  $1.0 \times 10^8 M_\odot$  of <sup>24</sup>Mg inferred to have been present in the galaxy if it had the solar composition<sup>8</sup> on the average when the Sun formed and if the total galactic mass is  $1.8 \times 10^{11} M_\odot$ . Howard *et al.*<sup>9,10</sup> have shown that the explosions of those same carbon shells produce many rare neutron-rich nuclei by subjecting the heavy elements present *ab initio* in the exploding star to the free-nucleon fluxes generated by the explosion of the carbon. Most of the significant radioactive yields at  $A=45, 47, 49, 50$  decay too quickly for gamma-ray astronomy. Although our publications do not tabulate the long-lived <sup>60</sup>Fe yield, it can be summarized as follows. A substantial fraction  $f_{60}(\text{Cr})$  of the seed chromium nuclei are driven to <sup>60</sup>Cr where they stop because of the intense rate of <sup>61</sup>Cr ( $\gamma, n$ ) <sup>60</sup>Cr. The exact fraction is uncertain because of uncertainty in the neutron separation energy from <sup>59</sup>Cr, but for this communication I estimate  $f_{60}(\text{Cr}) \approx 0.5$ . Because only about 1% of seed nuclei have been through explosive carbon burning events<sup>9,10</sup>, it follows that  $0.5 \times 10^{-2}$  of chromium has been converted to <sup>60</sup>Fe in exploding stars. By the preceding argument concerning <sup>24</sup>Mg, this amounts to  $1.0 \times 10^{-5} M_\odot$  of <sup>60</sup>Fe per average explosive event. This is not the source of <sup>60</sup>Ni in nature, the yield being inadequate by about two orders of magnitude.

The yield of <sup>60</sup>Fe can greatly exceed the estimate above, however, if a substantial portion of the iron seed nuclei can be converted to <sup>60</sup>Fe in the explosion. We found earlier<sup>9,10</sup> that the iron nuclei were driven largely to <sup>62</sup>Fe, with only about  $10^{-2}$  to  $10^{-4}$  of Fe remaining at <sup>60</sup>Fe. Such yields could at most double the <sup>60</sup>Fe yield quoted from Cr seed, so I shall take that value,  $1.0 \times 10^{-5} M_\odot$  <sup>60</sup>Fe, as a reasonable lower limit on the average explosive yield. There is quite another consideration, however, to be made from our indication that  $10^{-2}$  of all Fe have been through such events, because  $10^{-2}$  Fe exceeds by a factor of four the abundances of <sup>62</sup>Ni and <sup>64</sup>Ni, and is roughly half of the <sup>60</sup>Ni abundance. We must therefore pay close attention to the question of where this iron seed ends up. Only <sup>60</sup>Ni is sufficiently abundant among nuclei having  $A \geq 60$  to hide  $10^{-2}$  of the iron abundance. This suggests that a non-negligible fraction of <sup>60</sup>Ni may have been synthesized as <sup>60</sup>Fe from <sup>56</sup>Fe seed in explosive carbon burning. Although the Fe ( $n, \gamma$ ) flow reached <sup>62</sup>Fe in our exploratory calculations these do not conclusively eliminate the possibility that it is partly arrested at <sup>60</sup>Fe. The ( $\gamma, n$ ) lifetime of <sup>61</sup>Fe at  $T_9 = 2.15$  is only  $0.05 \text{ s}^{10}$ , much less than the expansion time. To stop

much of the flow at <sup>60</sup>Fe therefore requires only a somewhat smaller neutron flux than our calculations indicated. It would therefore be reasonable that the average <sup>60</sup>Fe yield was sufficient to produce about half of <sup>60</sup>Ni, in which case its value would be  $1.0 \times 10^{-3}$  of <sup>60</sup>Fe per explosive event. This value is two orders of magnitude greater than the yield expected from Cr seed, and I will regard it for the present discussion as a reasonable upper bound on the <sup>60</sup>Fe yield. If the integrated neutron flux is only about 1/3 of the values shown before<sup>10</sup>, and if the ( $n, \gamma$ ) cross-section of <sup>58</sup>Fe is actually quite small<sup>11</sup>, say 1/3 of that for <sup>56</sup>Fe, then this same explosion will also contribute a significant fraction of the natural <sup>58</sup>Fe abundance. This result may be significant in light of the difficulty<sup>12</sup> of synthesizing <sup>58</sup>Fe. This detail, though suggestive, need not concern us further here, except that it also suggests that the actual flow did not largely pass <sup>60</sup>Fe. It is the <sup>60</sup>Fe yield we are seeking.

Because these expectations depend on a situation in which most of <sup>62</sup>Ni and <sup>64</sup>Ni and part of <sup>60</sup>Ni are the results of Fe seed in explosive carbon burning, I should comment on the fact that <sup>60</sup>Ni and <sup>62</sup>Ni can be synthesized quite satisfactorily<sup>13</sup> in alpha-rich freezeouts following an *e*-process. The normalization of the alpha-rich freezeout to the alpha-depleted freezeout is not well established, although it fell naturally out of the <sup>12</sup>C-detonation model<sup>13</sup>. There is at present no convincing argument that the <sup>12</sup>C-detonation model is relevant in its fine details. Probably the only abundance requirement of the alpha-rich zones is the necessity to synthesize <sup>58</sup>Ni in the freezeout, but this can be accomplished without the associated synthesis of <sup>60</sup>Ni and <sup>62</sup>Ni. Thus it is quite possible, but it is by no means necessary, to synthesize <sup>60</sup>Ni and <sup>62</sup>Ni in the *e*-process freezeout. Because of the necessity of converting the Fe seed in explosive carbon burning into some nucleus, and because <sup>58</sup>Fe, <sup>60</sup>Ni and <sup>62</sup>Ni are the only plausible candidates, I prefer the view that <sup>62</sup>Ni, and perhaps a large part of <sup>60</sup>Ni, were synthesized in explosive carbon burning. The important difference for gamma-ray astronomy is that the <sup>60</sup>Ni synthesized in *e*-process freezeouts is synthesized as <sup>60</sup>Zn, whereas the <sup>60</sup>Ni synthesized in explosive carbon burning is synthesized as <sup>60</sup>Fe, the gamma-ray source in question. Thus in summary I take the explosive yield of <sup>60</sup>Fe to be  $1.0\beta \times 10^{-5} M_\odot$  per explosion, where  $\beta$  is a number near unity if virtually none ( $\leq 10^{-3}$ ) of the Fe seed can be arrested at <sup>60</sup>Fe in explosive carbon burning and is a number of order 100 if most of the Fe seed is arrested there. Thus  $1 < \beta < 100$ . Although this involved argument at present lacks numerical precision regarding the size of  $\beta$ , we can easily see that the result may be important for gamma-ray astronomy.

Let the distance in kpc to an explosive galactic remnant of age  $t$  be  $R$  (kpc). Then the flux of gamma rays having energy  $E_i$  at the Earth is

$$F_i = g_i \frac{\lambda N_0 e^{-\lambda t}}{4\pi R^2} \quad (1)$$

where  $N_0$  is the initial number of nuclei having mean lifetime  $1/\lambda$  and  $g_i$  is the number of gamma rays having energy  $E_i$  emitted per decay. For the <sup>60</sup>Fe yield estimated earlier we have, taking  $g_i = 1$  for the 1.17 MeV and 1.33 MeV lines<sup>6</sup>,

$$F_{60} = 1.3\beta \times 10^{-7} \frac{\exp(-t/4.3 \times 10^5 \text{ yr})}{R^2 (\text{kpc})} \text{ cm}^{-2} \text{ s}^{-1} \quad (2)$$

To be detectable from balloons a line flux should exceed  $2 \times 10^{-3} \text{ cm}^{-2} \text{ s}^{-1}$  (ref. 2) and to be detectable from satellites it should exceed  $4 \times 10^{-5} \text{ cm}^{-2} \text{ s}^{-1}$ . Thus if  $\beta = 1$  and we discuss only remnants less than  $10^5$  yr old we find that the remnants are detectable at 8 pc from balloons and 60 pc from satellites; however, if  $\beta = 100$  the detectability distances are ten times greater, thereby extending to almost 1 kpc from satellites. That this distance is quite large can be seen from considering that a circle of 1 kpc radius constitutes about 0.5% of the area of the galactic disk. At one per 30 yr, the galaxy has  $10^4$  explosive events in one <sup>60</sup>Fe half life, in which case



about fifty of them should be within 1 kpc distance. Thus a satellite could plausibly see many sources (if the angular size of the remnant falls within the field of view) and, if  $\beta = 100$ , a remnant at 0.1 kpc might even be detectable from a balloon (for example, Sco XR-1 is at roughly such a distance). These arguments suggest that a gamma-ray telescope with good energy resolution near 1 MeV might profitably look at nearby supernova remnants and pulsars.

It is notable that the 1.156 MeV line from the  $^{44}\text{Sc}$  decay in remnants could not be resolved from the 1.173 MeV line from the  $^{60}\text{Co}$  decay. Clearly the two possibilities will be distinguished by the equal intensity 1.33 MeV line in the  $^{60}\text{Co}$  case. For comparison the  $^{44}\text{Sc}$  flux<sup>2</sup> is expected to be

$$F_{44} = 2.1 \times 10^{-2} \frac{\exp(-t/68 \text{ yr})}{R^2 (\text{kpc})} \text{ cm}^{-2} \text{ s}^{-1} \quad (3)$$

which is five orders of magnitude greater than  $^{60}\text{F}$  immediately after the explosion, but decays much more rapidly. After 800 yr the  $^{60}\text{Fe}$  flux would exceed the  $^{44}\text{Sc}$  flux. Neither flux should be detectable from the Crab nebula, for instance, although if  $\beta = 100$  its  $^{60}\text{Fe}$  lines might be marginally detectable from a satellite.

I have been discussing these remnants as if they are resolvable in angle. Such may not be the case, of course. A remnant  $10^5$  yr old may well have a size of order 100 light years, in which case it would subtend a broad angle at a distance of  $10^3$  light years or closer. The radiation from such a source could appear as a general galactic background. Inasmuch as the average galactic luminosity in either  $^{44}\text{Sc}$  or  $^{60}\text{Fe}$  lines is expected to be of order  $10^{37} \text{ erg s}^{-1}$ , we can easily see that the galactic disk should always be visible in these lines. (I assume the disk to be transparent near 1 MeV.) The disk content of  $^{60}\text{Fe}$  is at all times equal to the number of atoms produced in the last mean lifetime of  $^{60}\text{Fe}$  (about  $1.3 \times 10^4$  explosive events). A reasonably wide angle telescope (I took a cone of half angle  $\pi/4$ ) viewing toward the galactic centre should then see a constant flux

$$F_{60} (\text{galactic}) \approx 1.7 \times 10^{-5} \beta \text{ cm}^{-2} \text{ s}^{-1} \quad (4)$$

which is marginal for  $\beta = 1$ , but an enormous signal if  $\beta = 100$ . By comparison again, the  $^{44}\text{Sc}$  flux would be

$$F_{44} (\text{galactic}) \approx 4.5 \times 10^{-4} \text{ cm}^{-2} \text{ s}^{-1} \quad (5)$$

for a time average of randomly positioned supernova remnants. In reality the mean lifetime of  $^{44}\text{Ti}$  (68 yr) means that the galactic background in  $^{44}\text{Sc}$  lines will depend on the actual age and locations of those explosive events within the past century. These are, of course, unknown. The  $^{56}\text{Co}$  lines<sup>2</sup>, which are even more intense, are also even more time-dependent and require observation of a specific event within about 1 yr of the explosion. As such, they constitute a very special case. Nonetheless, equations (4) and (5) suggest to me that a gamma-ray telescope with high energy resolution near 1 MeV could very advantageously observe the galactic centre from a satellite. Both  $^{44}\text{Sc}$  and  $^{60}\text{Fe}$  may be detectable, and the ratio of intensities at 1.33 and 1.16 MeV will be required to distinguish the two. The scientific benefit of any of these nuclear gamma-ray lines would be enormous—philosophically similar to the solar-neutrino equipment. As such it certainly merits a great effort.

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## Remote Sensing of Molecular Pollutants in the Atmosphere by Stimulated Raman Emission

ONE method of measuring atmospheric molecular pollutants at remote locations uses the Raman backscatter from a laser beam: the scattered wavelengths are characteristic of each pollutant, and their intensities give a measure of the respective concentrations<sup>1-3</sup>. But, because of the isotropic nature of the scatter, large telescopes are required to collect even a small fraction of the total scatter, and so the probing distance is small.

Here we propose a different method of using Raman interactions for pollutant determination. When a laser beam propagates through a mixture of gases, the molecules in the beam can provide signal amplification at any of the Raman wavelengths scattered. This phenomenon, the stimulated Raman effect, causes a signal of the proper wavelength propagating collinearly with the laser beam to be amplified proportionally to the concentration of the corresponding type of molecule in the mixture. And all of the amplified signal can be collected by a detector at the far end of the beam.

To calculate the signal gain, consider a volume  $V$  of the beam containing  $n_L(z)$  laser photons of wavelength  $\lambda_L$  and  $n_R(z)$  photons of wavelength  $\lambda_R$  equal to one of the Raman wavelengths of a certain molecular species in the atmosphere at location  $z$  along the direction of the beam. They travel in the same direction at the velocity of light  $c$ . The number of photons of wavelength  $\lambda_R$  scattered from the laser beam by the ordinary Raman effect per metre is

$$-\frac{dn_L(z)}{dz} \Bigg|_{\text{ordinary Raman, R}} = 8\pi \left( \frac{d\sigma}{d\Omega} \right) N(z) n_L(z) \quad (1)$$

where  $N(z)$  is the concentration (molecules per unit volume) of the scattering molecules at  $z$ , and  $(d\sigma/d\Omega)$  is their Raman cross section per unit solid angle and per direction of polarization for the  $\lambda_L$  to  $\lambda_R$  process.

These ordinary Raman photons fall into

$$\frac{8\pi V \Delta\nu}{c \lambda_R^2}$$

modes within the Raman line width  $\Delta\nu$ .

The stimulated Raman emission rate into one mode produced by each Raman photon present in it is equal to the ordinary Raman emission rate into the mode. So:

$$\begin{aligned} \frac{dn_R(z)}{dz} \Bigg|_{\text{stimulated Raman}} &= \left( \frac{8\pi (d\sigma/d\Omega) N(z) n_L(z)}{(8\pi/c \lambda_R^2) V \Delta\nu} \right) n_R(z) \\ &= g(z) n_R(z) \end{aligned} \quad (2)$$

where

$$g(z) = \left( \frac{c\lambda_R^2 N(z) (d\sigma/d\Omega)}{\Delta\nu} \right) \left( \frac{n_L(z)}{V} \right) \quad (3)$$

is the gain parameter at  $z$ . The  $n_L(z)$  photons may belong to several modes. At atmospheric pressure,  $\Delta\nu$  is mainly determined by the collision frequency<sup>4</sup>  $\nu_c = 6.9 \times 10^9$  Hz.

Numerically, for  $\lambda_L = 0.7 \mu\text{m}$  (ruby laser):

$$g \approx 2 \times 10^{16} (d\sigma/d\Omega) \zeta P_L (\lambda_R/\lambda_L)^2 \quad (4)$$

where  $g$  is the spatial gain in reciprocal metres,  $(d\sigma/d\Omega)$  is the scattering cross section in  $\text{M}^2$ ,  $\zeta$  is the molecular concentration in p.p.m., and  $P_L$  is the laser power density in  $\text{W m}^{-2}$ .

For example, for a 100 MW ruby laser pulse with  $100 \text{ cm}^2$  cross section propagating over a length  $L = 1 \text{ km}$  simultaneously with a Raman signal downshifted in frequency by the  $2,330 \text{ cm}^{-1}$  vibrational level of  $\text{N}_2$ , the signal intensity gain is obtained from (4) by using<sup>5</sup>  $d\sigma/d\Omega = 1.9 \times 10^{-31} \text{ cm}^2$ :  $e^{gL} \approx e^{4.4} = 81$ .

The same laser pulse accompanied by a signal downshifted by  $1,150 \text{ cm}^{-1}$  propagating through 1 kilometre of air containing  $\zeta = 1$  p.p.m. of  $\text{SO}_2$  with<sup>5</sup>  $d\sigma/d\Omega = 3 \times 10^{-31} \text{ cm}^2$  gives a gain

$$e^{gL} \approx 1 + gL \approx 1 + 10^{-5}$$

From a device point of view, the stimulated Raman method presents analogies with resonant absorption techniques based on beam attenuation measurements at allowed molecular transition wavelengths<sup>5</sup>. Instead of the tunable laser capability required by the latter, it involves a Raman signal source which may consist either of another tunable, low-power laser or of a Raman cell located in the primary beam. Both methods give average concentrations over a path and necessitate equipment at two locations.

One advantage of stimulated Raman is the possibility of compensating for spurious scattering effects (turbulence, Rayleigh, and so on, which are larger than either Raman gain or resonant attenuation alone) by using pairs of pulses in fast succession with the same Raman content but with laser photons in one of them only: the difference between Raman photon counts at the detector measures the stimulated emission. Another advantage is the possibility of using resonant Raman<sup>6</sup> which enhances the cross section for the pollutant molecules alone, thus compensating for their typically small concentration compared with  $\text{N}_2$  and  $\text{O}_2$ .

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## Explosive or Detonative Phase Transitions on a Geological Scale

IN both the geological and chemical literature<sup>1-6</sup> the possibility has been suggested that rapid polymorphic transitions provide a source mechanism for medium to deep focus earthquakes. The properties, and in particular the sign of the volume change, of "laboratory scale" rapidly phase transforming systems have

not previously been considered with a view to providing further information on the proposed geological systems. There are a number of reported cases, some of which<sup>7-17</sup> are listed in Table 1, of transitions from a metastable state to a stable state accompanied by acoustic phenomena or shock waves. The sign of the density change during phase transition may be either positive (for example, crystallization of nickel<sup>8</sup>) or negative (for example, the phase change of explosive antimony<sup>10,11</sup>).

In nature, a phase change may take place in volumes of the order of cubic kilometres. This change would spread through the volume accompanied by a shock wave. In fact for this mechanism to operate, the transition must be induced by the shock wave and the transition itself must add energy to the shock wave. Although the energy per gram is small, because the total mass is large the total energy released might be very large indeed.

Table 1 Rapid Phase Transformations

| System                                       | Ref.  |
|----------------------------------------------|-------|
| Crystallization from supersaturated solution |       |
| Water-sulphite                               | 7     |
| Crystallization from supercooled melt        |       |
| Nickel                                       | 8     |
| Bismuth                                      | 9     |
| Polymorphic transitions                      |       |
| Antimony                                     | 10    |
| Arsenic                                      | 12,13 |
| Iron, austenite $\rightarrow$ martensite     | 14    |
| Lead azide, $\beta \rightarrow \alpha$       | 15    |
| Polymerization                               |       |
| Maleic anhydride                             | 16,17 |

Modern theories of continental drift and plate tectonics involve the sinking of material at continental margins (see, for example, ref. 18). This may provide the mechanism for the attainment of a metastable state prior to explosive transformation to a denser phase. An explosive transition to a less dense phase is also possible, associated with upward convection<sup>6</sup>. Although the current geophysical view is that earthquake sources are characterized by shear phenomena rather than by solid-solid transitions with volume change<sup>19</sup>, the existence of a number of explosive or detonative phase transformations in chemical systems suggests that such a process should not be dismissed as a possible earthquake source mechanism. Indeed, deep focus earthquakes occur at depths up to 700 km in regions (New Zealand, for example) where one might expect, from modern theories of continental drift, the ocean floor to be pushed under a continental plate. At a depth of 700 km the temperature and pressure conditions are such that earthquakes caused by shear phenomena are difficult to imagine, and a mechanism such as the one postulated is required.

Finally, it should be pointed out that these adiabatic processes bear a close relationship to the isenthalpic decompressions suggested by Waldbaum<sup>20</sup> for the volcanism of seismically active zones.

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## Plate Consumption: Some Comments from an Island Arc

A COROLLARY of lithospheric plate creation on an Earth of stable radius is that plate is also consumed. The island arcs with their phenomena of intense geophysical disturbance form the logical sinks, and the subduction theory of geosynclines has recently been added to the theory of plate consumption at island arcs.

I offer some comments about the difficulties I have in accepting, in its present form, the "dogma" of plate tectonics and its less well based extension to geosynclinal theory.

An "earthquake occurs when the stresses in some region inside the Earth have accumulated to the point of exceeding the strength of the material in some place, leading rapidly to fracture"<sup>1</sup>. This being so, the absence of earthquakes can be attributed, on the one hand, to absence of appropriate stress or, on the other hand, to either extremely high or extremely low strength of the rock.

In the oceanic plates earthquakes do not occur between the mid-ocean ridge/transform fault systems and the allegedly downgoing part of the plate at some depth to the continental side of a trench<sup>2</sup>. Scholz<sup>3</sup> has supposed "that the deviatoric stresses which are directly responsible for earthquakes must occur quite widely and cannot be restricted to seismic zones" and this has led to his suggestion of variable rock strength based on chemical composition. This does not, however, explain why earthquakes occur in the oceanic plates near the mid-ocean ridges but not again in the plate until that plate is plunging into the mantle, unless major changes in the nature of the oceanic plate occur.

The recent data in support of plate tectonics imply that the sub-ocean mantle-crust system consists of a lithosphere about 100 km thick which behaves mechanically like a rigid (high strength) plate. Press<sup>4</sup> attributes a higher density to this rigid plate than to the mantle beneath it, and presumably the strength of the plate is sufficient to contain the buoyancy forces in this inherently unstable situation. But the paradox of the behaviour of this remarkably rigid plate at the plate boundary must also be recognized<sup>5</sup>. Here the plate plunges into the mantle, bending at angles of between 15° and 90° to do so, and it might be expected that the internal stresses involved would be considerable if the plate is rigid. Yet few earthquakes occur in the oceanic lithosphere beneath a trench<sup>2</sup>.

Assumption of an extremely weak sub-oceanic lithosphere, while possibly explaining the lack of earthquakes beneath the ocean floor and at the trench, removes the explanation for earthquakes at the mid-ocean ridge and in the Benioff zone. Further, such a mechanically weak plate could not produce the stress situations behind the island arcs which lead to the greater part of the world's seismicity. The conclusion must be that "the deviatoric stresses directly responsible for earthquakes" do not occur widely.

A negative gravity anomaly is a principal characteristic of the asymmetric active region<sup>6,7</sup>. The coincidence of the negative gravity anomaly and the trench of an active system was widely accepted for many years, but in recent work<sup>6,8</sup> I have stressed that the negative gravity anomalies do not necessarily coincide with the trench but may occur also on the inner side of the trench.

As a development of the plate tectonic hypothesis, it has been suggested that deformed "eugeosynclinal" sediments of the Franciscan type have accreted at plate junctures by subduction<sup>9-11</sup> and that the negative gravity anomalies are the result of the accumulating sediments<sup>10,12</sup>. One of the reasons for linking the negative gravity anomalies with the accumulating sediments is that "the strong negative gravity anomalies . . . are generally displaced towards the island arc"<sup>10</sup>. Thus the pendulum has swung from coincidence of negative gravity anomaly with the trench to the generalization that the anomaly is displaced from the trench.

In fact the axis of negative gravity anomalies may be: (a) coincident with or within 20 km of that of the trench (Tonga, Kermadec, Chile); (b) displaced towards the inside of the active system by a small distance (Sunda, Japan), or (c) displaced quite large distances from the trench (Hikurangi (New Zealand) 200 km, Kamchatka 100 km). While the transport of low density sediments to inconvenient distances under New Zealand and Kamchatka could respond to special pleading, it would appear impossible to explain the gravity anomalies which are coincident with the trench in terms of sediment accretion under the continental slope.

A further point which must be noted is that the amplitudes of the negative gravity anomalies tend to be independent of the rate or direction of relative motion of plates. The greatest negative anomaly is coincident with the Puerto Rico Trench where "the motion is nearly parallel to the strike of the trench"<sup>5</sup>.

Whereas the mantle seismicity, volcanism, gravity and heat flow anomalies of island arcs are spatially related in a quite rigorous manner<sup>6,8,13</sup>, the position of the trench is not invariant in its geometrical relationship with other features of an island arc<sup>6</sup>. The special problem of the trench is the conflict between the theoretically compressional nature of plate boundaries and the apparently tensional nature of the trench itself. Indeed why the collision of two "rigid" plates should produce a trench is so far unexplained. Such earthquakes as are observed beneath trenches have a fault-plane solution best satisfied by normal faulting<sup>14,15</sup>. A sub-oceanic lithosphere must thus be fracturing and sinking through excess mass rather than by thrusting against its continental neighbour—but then the cause of seismicity and shortening in the continental-side crust is unknown. Elsasser<sup>16</sup> has restated the case for tension at the trenches, a point obscured in many recent discussions, and has pointed out the difficulties in developing a dynamical model for plate consumption.

From a study of the geology and geophysics of geosynclines I concluded that the ultramafic belts, rather than being a product of orogeny, as was then believed, actually predated sedimentation in the geosynclinal pair<sup>17</sup>. This view is now commonly held<sup>18,19</sup>. I also suggested<sup>17</sup> that the Macquarie Ridge was the ultramafic belt of an incipient geosynclinal pair, an idea later developed by the proponents of subduction as a geosynclinal precursor<sup>11</sup>. But these later workers have evolved from the simplicity of plate tectonics a series of "catastrophic" mechanisms of great complexity to explain the emplacement

of ultramafic belts<sup>9</sup>. I would like to draw attention once again to the remarkable geophysical and geological similarity between the New Zealand Geosyncline and the Franciscan–Great Valley system<sup>17</sup>. Further, a comparison of Kay's restored Appalachian cross-section<sup>20</sup> with the New Zealand and Californian systems shows that all three are laterally dimensionally equivalent. This indicates that the formation of geosynclines is based on a physico-chemical process rather than catastrophic collision.

Two methods of ultramafic emplacement, the subducted and the obducted, are propounded by the plate tectonicists. There seems, however, to be no independent method of identifying obducted or subducted ultramafics. The Papuan ultramafic belt is quoted<sup>11</sup> as an obducted type and it is alleged that superposed calc-alkaline volcanics are few or absent. But andesitic volcanoes of high potash type are at present active parallel to part of the belt and there are many late Pleistocene to recent volcanic centres north of the ultramafic belt. There is no Benioff zone beneath the andesite volcanoes of Papua, but the conjunction of high potash andesite volcanoes and ultramafics in California, New Zealand and Macquarie Ridge has been noted previously<sup>17</sup> and has led to the suggestion of "mio-geosynclinal" andesites<sup>21</sup> which are in all cases a similar distance from ultramafic belts<sup>13</sup>. The "orogenic" and "mio"-andesites may well be derived by independent processes.

None of the collision models explains the marginal or mio-geosynclines, which everywhere are of similar width (~100 km) and of similar structure. Ernst<sup>22</sup> has given the most serious treatment of the development of the Great Valley sequence, but again it is difficult to envisage the cataclysmic nature of collision tectonics producing such simple and similar structures. A relaxation rather than a perturbation process would seem more appropriate for the formation of mio-geosynclines.

If seafloor spreading is accepted, and there are few dissenters, it is required that either the Earth expand or lithosphere be consumed. There are, however, objections to many of the dynamical implications of presently inferred plate kinematics in the consuming regions. In particular, the concept of subduction as the precursor of a "geosyncline" appears to me untenable and indeed there is grave doubt that geosynclines are the perturbation products of island arcs. The introduction of subducted and obducted ophiolite suites merely reflects this uncertainty.

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## BIOLOGICAL SCIENCES

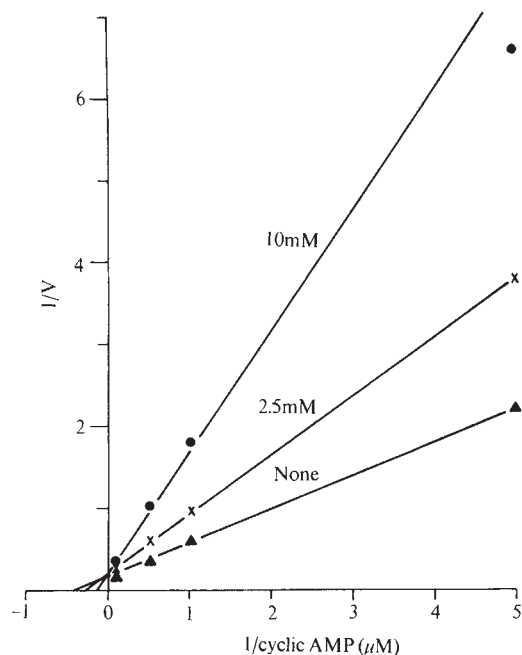
### Inhibition of Cyclic 3',5'-AMP Phosphodiesterase in Islet Cells and Other Tissues by Tolbutamide

AN explanation of the intracellular mechanism by which sulphonylureas such as tolbutamide and chlorpropamide stimulate the release of insulin from the pancreas<sup>1,2</sup> (as in the treatment of diabetes mellitus) should also account for certain effects of these materials outside the pancreas<sup>3-5</sup>. These include the effect of chlorpropamide in reducing polyuria in patients with diabetes insipidus<sup>4</sup> and the possible involvement of tolbutamide in the increased cardiac mortality of patients treated with the drug<sup>5</sup>. It has been clear for some years that cyclic AMP (cyclic adenosine 3',5'-monophosphate) plays an important part in the regulation of insulin secretion. For example, cyclic AMP causes the release of insulin from pancreas tissue *in vitro*<sup>6</sup>. Further, glucagon increases cyclic AMP activity in islet cells by activating the enzyme adenylyl cyclase<sup>7</sup>, and theophylline increases cyclic AMP activity by inhibiting phosphodiesterase<sup>8,9</sup>. It is known that the sulphonylureas can influence the activity of cyclic AMP<sup>10,11</sup>, and tolbutamide and chlorpropamide inhibit the enzyme phosphodiesterase in the rat kidney<sup>12</sup>. We have now shown that the sulphonylureas may regulate the levels of cyclic AMP not merely in islet cells but in kidney, lung, heart and brain. In particular, the sulphonylureas, like the methylxanthines, inhibit the activity of cyclic AMP phosphodiesterase, which hydrolyses cyclic AMP to 5'-AMP<sup>8,9</sup>, which provides a plausible explanation for their effects in the pancreas and elsewhere.

We studied cyclic AMP phosphodiesterase activity in a tolbutamide-sensitive insulin-secreting islet cell tumour of the Syrian golden hamster (*Mesocricetus auratus auratus*)<sup>13</sup>, which also contains glucagon-sensitive adenylyl cyclase (manuscript in preparation). Tumours, 2–5 cm in diameter, were removed, homogenized in 10 volumes of 0.3 M sucrose with 2 mM MgCl<sub>2</sub> and centrifuged at 10,000g for 10 min. More than 80% of the phosphodiesterase activity and no detectable 5' nucleotidase activity was in the 10,000g supernatant fraction which was brought to 50% saturation with solid ammonium sulphate. The precipitate was then dissolved in 0.1 M Tris HCl (pH 7.5) containing 1 mM EDTA, dialysed against this buffer, and stored at –60° C until use.

Phosphodiesterase activity was measured by the hydrolysis of <sup>3</sup>H-cyclic AMP. Unless otherwise stated, the incubation mixture contained 0.1 M Tris HCl (pH 6.8), 2.5 mM MgCl<sub>2</sub>, 0.1 mM EDTA, 0.001 mM <sup>3</sup>H-cyclic AMP (10,000–20,000 c.m.p.) and 21 µg of islet cell phosphodiesterase, in 0.1 ml. Reactions were begun by the addition of phosphodiesterase. After incubation for 20 min at 37° C, ZnSO<sub>4</sub> and Ba(OH)<sub>2</sub> were added to stop the reaction and to precipitate the <sup>3</sup>H-5'-AMP formed by the action of the phosphodiesterase<sup>14</sup>; the unhydrolysed <sup>3</sup>H-cyclic AMP in the supernatant fraction was measured by liquid scintillation. Phosphodiesterase activity was calculated from the decrease in <sup>3</sup>H-cyclic AMP. After prolonged treatment with islet cell phosphodiesterase, more than 90% of the radioactivity was precipitated by ZnSO<sub>4</sub>–Ba(OH)<sub>2</sub>. Further, in several experiments, the radioactivity remaining in the ZnSO<sub>4</sub>–Ba(OH)<sub>2</sub> supernatant after phosphodiesterase treatment either with or without tolbutamide was identified as <sup>3</sup>H-cyclic AMP by chromatography on 'Dowex-50'. We used 10<sup>–6</sup> M cyclic AMP in the phosphodiesterase assay in order to approximate the intracellular concentration of the nucleotide. The apparent K<sub>m</sub> for cyclic AMP of our phosphodiesterase preparation is about 2 × 10<sup>–6</sup> M, so that phosphodiesterase activity in these conditions was not strictly linear with time. We therefore studied phosphodiesterase activity only in the range of 0–40% hydrolysis of the <sup>3</sup>H-cyclic AMP, within which range the reaction rate was approximately linear with time and with enzyme concentration. Although we studied





**Fig. 1** Effect of tolbutamide concentration on hamster tumour phosphodiesterase activity. The ordinate,  $1/V$ , is the reciprocal of enzyme velocity, expressed as pmol cyclic AMP hydrolysed/min.

phosphodiesterase activity at pH 6.8, inhibition was similar at 7.0, 7.5 and 8.0.

Tolbutamide produced substantial inhibition of phosphodiesterase activity (Table 1). Another sulphonylurea, 1-butyl-3-[(*p*-acetyl phenyl) sulphonyl]<sup>2</sup> urea, had a similar effect. Both sulphonylureas were slightly less active than the methylxanthines, caffeine and theophylline. This inhibitory action depended on the intact sulphonylurea structure because a mixture of the sulphonic acid and urea moieties of tolbutamide had little inhibitory activity. The inhibition of phosphodiesterase activity by tolbutamide seemed to be competitive (Fig. 1). In several experiments, the  $K_i$  was about 3 mM. Although this phosphodiesterase is an  $Mg^{2+}$ -dependent enzyme, the inhibition by tolbutamide was independent of  $Mg^{2+}$ , for inhibition by 8 mM tolbutamide was identical for the range of 2.5–10 mM  $MgCl_2$ .

Other studies revealed that tolbutamide inhibited phosphodiesterase activity in preparations of rat brain, liver, lung, and kidney (Table 2). To show more clearly that tolbutamide interacted directly with the phosphodiesterase, purified heart phosphodiesterase (Boehringer) was tested. Tolbutamide also inhibited this purified enzyme, with a  $K_i$  similar to that found with the islet cell preparation (3 mM) (not shown).

Inhibition of phosphodiesterase activity by sulphonylureas provides an attractive explanation of their insulinogenic action. Although the plasma concentrations produced by standard oral doses of tolbutamide in man (0.3–0.7 mM)<sup>3,15</sup> cause only slight inhibition of phosphodiesterase activity *in vitro*, nothing is known about the rapidity of uptake or the intracellular concentration of sulphonylureas in  $\beta$ -cells. Also, Beavo *et al.* demonstrated that methylxanthines produce significant lipolysis

**Table 2** Inhibition of Phosphodiesterase by Tolbutamide in Rat Tissues

| Tissue | Protein ( $\mu$ g) | Nmol cyclic AMP hydrolysed/20 min/ $\mu$ g protein |                    |
|--------|--------------------|----------------------------------------------------|--------------------|
|        |                    | Without tolbutamide                                | Tolbutamide (5 mM) |
| Brain  | (3)                | 163                                                | 90                 |
| Kidney | (7)                | 83                                                 | 53                 |
| Lung   | (6)                | 71                                                 | 35                 |
| Liver  | (4)                | 22                                                 | 13                 |

In this experiment 1 g of tissue was homogenized in ten volumes of 0.3 M sucrose with 0.001 M  $MgCl_2$  and then centrifuged at 10,000g for 10 min. The supernatant was dialysed against 0.1 M Tris (pH 7.5) with 0.001 M EDTA for 12 h, then used in the phosphodiesterase assay.

in adipose tissue at concentrations which cause less than 20% inhibition of phosphodiesterase activity<sup>16</sup>. Further, small doses of theophylline can increase plasma insulin levels in man<sup>8</sup>. But phenothiazines<sup>17</sup>, reserpine<sup>17</sup>, thiazidiazines<sup>18</sup>, diazoxide<sup>18</sup>, papaverine<sup>19</sup>, and triiodothyronine<sup>20</sup> all also inhibit phosphodiesterase activity. The significance *in vivo* of phosphodiesterase inhibition is unclear. Sulphonylureas do not have striking effects in most extra-pancreatic tissues, and so these compounds may not readily enter, or may be rapidly inactivated in, most other tissues. Further, tolbutamide may have actions other than phosphodiesterase inhibition. In contrast to theophylline, tolbutamide inhibits lipolysis in adipose tissue. The ability of tolbutamide to uncouple oxidative phosphorylation has been proposed as the mechanism of its anti-lipolytic action<sup>21</sup>. Studies of the effect of sulphonylureas on tissue cyclic AMP levels will be required to test the hypothesis that these compounds produce significant inhibition of phosphodiesterase activity *in vivo*; such studies are now in progress in our laboratory.

We conclude that sulphonylureas, like methylxanthines, can inhibit cyclic AMP phosphodiesterase activity in many tissues. In kidney, inhibition of phosphodiesterase activity may lead to potentiation of vasopressin action and increased urinary excretion of cyclic AMP<sup>10–12</sup>. Inhibition of phosphodiesterase activity in  $\beta$ -cells may be the mechanism by which sulphonylureas stimulate insulin secretion.

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*Note added in proof.* Since this manuscript was submitted for publication, the work of Chaudhuri and Wines (*J. Lab. Clin. Med.*, **76**, 863; 1970) has come to our attention. These workers were the first to report that sulphonylureas inhibit cyclic AMP phosphodiesterase.

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**Table 1** Inhibition of Tumour Phosphodiesterase by Methylxanthines and Sulphonylureas

| Addition (10 mM)                                      | Phosphodiesterase activity remaining (%) |
|-------------------------------------------------------|------------------------------------------|
| None                                                  | 100                                      |
| Caffeine                                              | 18                                       |
| Theophylline                                          | 31                                       |
| 1-Butyl-3-[( <i>p</i> -acetyl phenyl) sulphonyl] urea | 43                                       |
| Tolbutamide                                           | 47                                       |
| <i>p</i> -Toluenesulphonic acid + butylurea           | 85                                       |

Hamster tumour phosphodiesterase was assayed in the presence of the compounds listed above; enzyme activity is expressed as the percentage of activity found in the absence of inhibitors.

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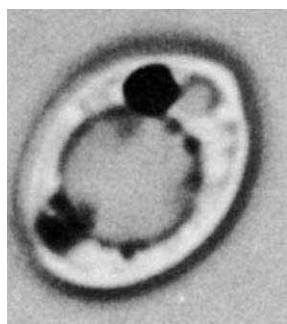
## Thallium as a Vital Stain for Yeast Mitochondria

BLACK, hexagonal crystals of thallium oxide,  $Tl_2O_3$  (identified by their shape and colour), appear in some cells of some strains of *Saccharomyces cerevisiae* when the culture is grown in nutrient medium containing 125 p.p.m. of thallium sulphate. The black, hexagonal plates of  $Tl_2O_3$  lie in a space between the cell wall and the plasma membrane. Usually only one large crystal is present, but as many as six small ones may appear; all are outside the protoplast. When a crystal is viewed from the edge, a bulge in the cell wall appears right over the crystal. The large crystals bend to conform to the curvature of the cell (Fig. 1).

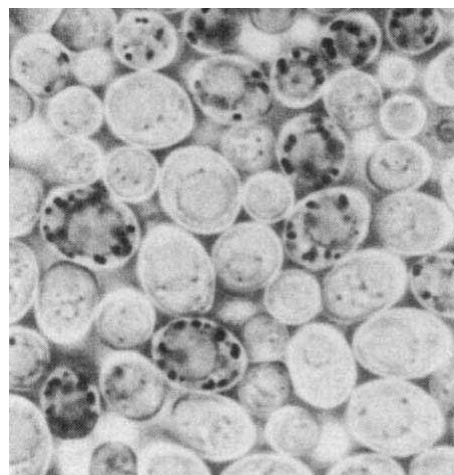
The cristate mitochondria are not visible in the living yeast cell and are difficult to fix and stain, but Mr Michael Todd, working with Dr Hewson Swift, has established (by an original staining method, personal communication) that a single layer of regularly arranged, cristate mitochondria (almost in contact with each other) lies in contact with the inner surface of the plasma membrane of the yeast cell.

In some of the cells grown in thallium, as many as forty cristate mitochondria are stained black by thallium (Fig. 2). The layer of irregularly elliptical, black-stained, cristate mitochondria lies against the inner layer of the plasma membrane. In some mitochondria only a small black dot of stain appears.

A layer of lipolated mitochondria covers the outer surface of the nuclear membrane and the spindle-reservoir of the yeast cell<sup>1</sup>. The lipolated mitochondria darkened by refraction can easily be distinguished from the jet-black, thallium-stained,



**Fig. 1** A yeast cell containing two flat, black, hexagonal crystals of thallium oxide fitted between the cell wall and the plasma membrane. Only one crystal is in focus. Lipolated mitochondria darkened by refraction and others that are bright and refractile lie on the surface of the nuclear vacuole.



**Fig. 2** A culture in which some cells contain a single layer of thallium-stained, cristate mitochondria lying against the inner wall of the plasma membrane.

cristate mitochondria (Fig. 2). The lipolated mitochondria do not take up thallium.

When cells grown in thallium are spread on nutrient agar, the unaffected cells bud rapidly and produce colonies.

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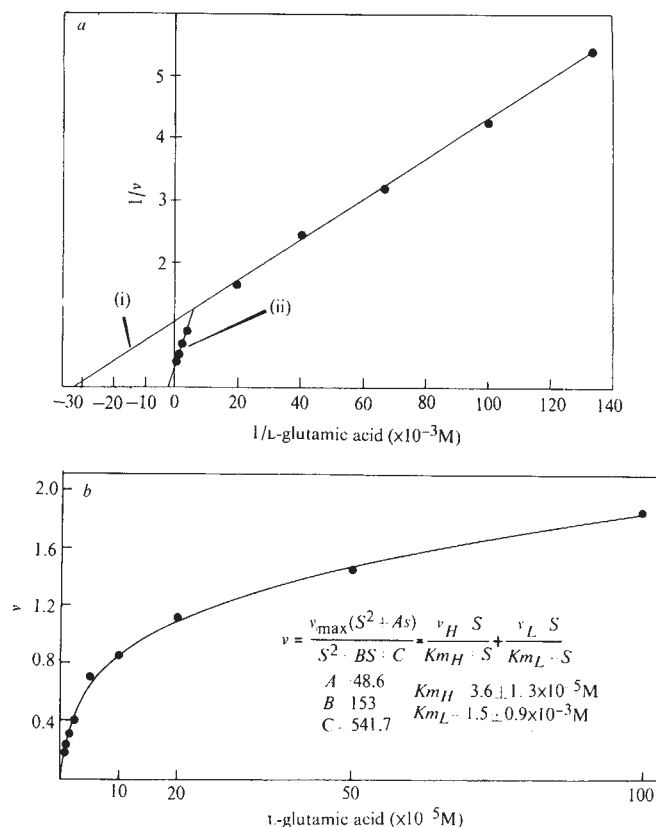
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## Unique High Affinity Uptake Systems for Glycine, Glutamic and Aspartic Acids in Central Nervous Tissue of the Rat

NEUROPHYSIOLOGICAL experiments suggest that several amino-acids, especially glycine and glutamic, aspartic and gamma-aminobutyric (GABA) acids, are neurotransmitters in the central nervous system<sup>1-3</sup>. But careful subcellular<sup>4,5</sup> and regional studies<sup>6-8</sup> have not provided supporting neurochemical evidence, because amino-acids, with the possible exception of GABA, also have general metabolic functions in nervous tissue.

Biochemical studies of transmitters in the central nervous system have been facilitated by the discovery of specific uptake systems into neurones with selective high affinities for various putative transmitters<sup>9-14</sup>. There is evidence that such re-uptake mechanisms peripherally inactivate synaptically released catecholamines<sup>9</sup> and it has been postulated that they account for the synaptic inactivation of other neurotransmitters. If amino-acids are neurotransmitters, they might be accumulated into neurones by similar selective uptake. Transport systems have been described for amino-acids in brain tissue, which, however, have relatively low affinity<sup>15</sup>. For this reason and because these transport systems exist for all amino-acids in numerous tissues outside the central nervous system, it is not likely that they have a specific function in inactivating postulated "neurotransmitter pools" of certain amino-acids. Uptake systems for hypothetical "transmitter pools" of amino-acids might be anticipated to have more affinity for their substrates than do general amino-





**Fig. 1** *a*, Double-reciprocal plot of velocity ( $v$ ) of L-glutamic acid accumulation into homogenates of rat cerebral cortex at different concentrations. Each point is the mean of four determinations. Velocity is expressed as  $\mu\text{mol}$  of L-glutamic acid/g/4 min. (i) High affinity system ( $Km_H = 3.6 \pm 1.3 \times 10^{-5} M$ ); (ii) low affinity system ( $Km_L = 1.5 \pm 0.9 \times 10^{-3} M$ ). *b*, Computed least squares fit of the same velocity of L-glutamic acid uptake data as in *a*, assuming transport by a two component system. Analysis of these data assuming a one component system gave a poor fit.

acid transports. Demonstration of such high affinity transport systems for physiologically active amino-acids would provide neurochemical support for their transmitter function.

We have examined the accumulation of several amino-acids into synaptosomes (pinched off nerve terminals) of rat cerebral cortex and spinal cord. All of them were accumulated by low affinity saturable mechanisms as in brain slices<sup>15</sup>. In addition, L-glutamic and L-aspartic acids in the cerebral cortex and spinal cord and glycine in the spinal cord had unique high affinity uptake systems.

Male Sprague-Dawley rats (150–200 g) were decapitated; the cerebral cortex or lumbosacral spinal cord were rapidly dissected and homogenized in 20 volumes of ice-cold 0.32 M sucrose in a Potter-Elvehjem glass homogenizer fitted with a 'Teflon' pestle (0.004–0.006 inch clearance). After centrifugation at 1,000g for 10 min, the pellet was discarded and a 0.2 ml. aliquot of the supernatant fluid was added to 3.8 ml. of Krebs-Henseleit bicarbonate solution with 11.1 mM glucose, containing varying concentrations of radiolabelled L-amino-acids and agitated at 37° C under  $O_2$ - $CO_2$  (95 : 5) for 4 min. After the incubation mixture had been centrifuged at 27,000g for 15 min, radioactivity in the supernatant fluid was assayed. The pellets were washed with ice-cold 0.9% NaCl, recentrifuged at 48,000g for 10 min at 4° C and accumulated radioactivity was extracted into 'Triton X-100':toluene phosphor and assayed by liquid scintillation spectrometry. Paper chromatographic analysis in three systems revealed that for all amino-acids examined, more than 90% of the radioactivity in the pellets was unmetabolized. Essentially no radioactivity remained in perchloric acid precipitates of the pellets, suggesting negligible incorporation of the amino-acids into protein under

these conditions. The accumulation of radioactivity was linear for at least 4 min and with varying concentrations of tissue homogenates.

When pellets obtained from these incubations were layered on continuous sucrose density gradients (0.32–1.5 M) and centrifuged for 90 min<sup>10</sup>, the accumulated amino-acids were localized to an area of the gradient enriched in synaptosomes<sup>10</sup> and separable from the profile of monoamine oxidase activity, a mitochondrial marker. Hypotonic shock completely liberated the particulate amino-acids. These observations indicated that the  $^3H$ -amino-acids were accumulated predominantly by synaptosomes.

$^3H$ -L-amino-acids (specific activity 1–5 Ci/mmol) were obtained from the New England Nuclear Corporation. The accumulation of amino-acids was examined with 6–10 concentrations of amino-acids ranging from  $10^{-8}$  to  $2 \times 10^{-3} M$ . Kinetics of uptake were determined by double-reciprocal plots<sup>16</sup> and by least squares fitting of a substrate-velocity curve to the data with computer programs donated by W. W. Cleland which provided  $Km$  values and their standard errors<sup>17</sup>.

In the cerebral cortex, double reciprocal plots for alanine, arginine, leucine, serine and glycine described straight lines with positive ordinate intercepts. Single transport system curves gave the best fit for these results, with  $Km$  values of  $0.2$ – $0.8 \times 10^{-3} M$  (Table 1). Double reciprocal plots for the accumulation of glutamic and aspartic acids, however, produced curves (Fig. 1*a* and *b*) which could be resolved into two components graphically as well as by computer analysis with calculated  $Km$  values of  $2$ – $3 \times 10^{-5} M$  and about  $0.4$ – $1.5 \times 10^{-3} M$  for the high and low affinity uptake systems respectively. We found a similar dual affinity uptake system for L-glutamic acid in rat cerebral cortical slices.

In the spinal cord, double reciprocal plots for the accumulation of alanine, arginine, leucine and serine yielded single straight lines with  $Km$  values of  $1$ – $9 \times 10^{-3} M$ . Those for glutamic and aspartic acids and glycine, however, described curves that could be resolved into two components. The high affinity transports for these three amino-acids had  $Km$  values of about  $2 \times 10^{-5} M$ , while the low affinity uptake  $Km$  values were about  $4 \times 10^{-3} M$ ,  $2 \times 10^{-3} M$ , and  $1 \times 10^{-3} M$  for aspartic acid, glutamic acid and glycine respectively (Table 1).

Glutamic and aspartic acids are similar chemically and their uptake in cerebral cortex and spinal cord is kinetically similar, and so they may be accumulated by the same transport system. We therefore assessed the mutual inhibition of glutamic and aspartic acid uptake by two kinetic techniques. In the Lineweaver-Burk technique<sup>16</sup> varying amounts of one  $^3H$ -L-amino-acid were incubated with a fixed amount of the other unlabelled amino-acid, while in the method of Dixon<sup>18</sup> two

**Table 1** Kinetics of Amino-acid Uptake into Homogenates of Rat Central Nervous System

| Amino-acid      | $Km$ values $\times 10^{-4} M$ |                      |
|-----------------|--------------------------------|----------------------|
|                 | Low affinity uptake            | High affinity uptake |
| Cerebral cortex |                                |                      |
| L-Aspartic acid | $3.68 \pm 1.94$                | $0.169 \pm 0.128$    |
| L-Glutamic acid | $14.97 \pm 9.59$               | $0.362 \pm 0.129$    |
| Glycine         | $7.63 \pm 0.53$                | —                    |
| L-Alanine       | $4.53 \pm 0.24$                | —                    |
| L-Arginine      | $6.24 \pm 1.11$                | —                    |
| L-Leucine       | $3.51 \pm 0.47$                | —                    |
| L-Serine        | $2.16 \pm 0.35$                | —                    |
| Spinal cord     |                                |                      |
| L-Aspartic acid | $37.62 \pm 14.17$              | $0.215 \pm 0.077$    |
| L-Glutamic acid | $16.73 \pm 6.48$               | $0.189 \pm 0.058$    |
| Glycine         | $9.23 \pm 6.38$                | $0.265 \pm 0.141$    |
| L-Alanine       | $10.58 \pm 0.71$               | —                    |
| L-Arginine      | $26.79 \pm 7.58$               | —                    |
| L-Leucine       | $91.36 \pm 30.18$              | —                    |
| L-Serine        | $10.81 \pm 2.06$               | —                    |

$Km$  values and their standard errors were determined as described in the text. A minimum of six concentrations of every amino-acid were incubated in quadruplicate with homogenates.

concentrations of the labelled amino-acid were used with varying concentrations of the other unlabelled amino-acid. In both cerebral cortex and spinal cord, mutual inhibition of glutamic and aspartic acid uptake was competitive. With both techniques, the  $K_i$  value for inhibition of  $^3\text{H}$ -glutamic acid uptake in the cerebral cortex and spinal cord by aspartic acid was  $3 \times 10^{-5}$  M. The  $K_i$  values for inhibition of  $^3\text{H}$ -aspartic acid accumulation by glutamic acid in cerebral cortex and spinal cord were  $3.5 \times 10^{-5}$  M and  $4.0 \times 10^{-5}$  M respectively. The competitive mutual inhibition of the uptake of glutamic and aspartic acids and the similarity of  $K_i$  and  $K_m$  values indicate that these two amino-acids can share the same transport system.

The active uptake of GABA, a metabolite of glutamic acid, into brain slices has a  $K_m$  value of about  $3 \times 10^{-5}$  M<sup>14</sup>. GABA concentrations as high as  $10^{-3}$  M, however, failed to reduce synaptosomal  $^3\text{H}$ -glutamic acid uptake, indicating that neither the low nor high affinity glutamic acid transports involve GABA neurones.

Our results show that representatives of large and small neutral and basic amino-acids are accumulated by synaptosomes in the cerebral cortex and spinal cord by mechanisms resembling their uptake in rat brain slices<sup>11</sup> and other tissues. Glutamic and aspartic acids and glycine utilize, in addition, unique high affinity uptake systems in synaptosomes and brain slices. Hammerschlag, Potter and Vinci independently reported a similar uptake of glutamic acid in rat spinal cord slices<sup>19</sup>. Interestingly, glutamic and aspartic acids, which are neurophysiologically active both in cerebral cortex and spinal cord, showed high affinity uptake systems in both of these regions. Recently, we described a unique population of synaptosomes in the cerebral cortex which selectively accumulate glutamic and aspartic acids, conceivably by the unique uptake system described here<sup>20</sup>.

Glycine, which mimics the behaviour of the natural inhibitory transmitter in the spinal cord but not in the cerebral cortex<sup>21</sup>, in this study was transported by a high affinity uptake system in the spinal cord but not in the cerebral cortex. Earlier, Neal and Pickles<sup>22</sup> described a high affinity uptake of glycine in rat spinal cord slices, and Johnston and Iversen<sup>23</sup> have independently observed a high affinity transport of glycine in slices and homogenates of rat spinal cord and medulla oblongata but not in the cerebral cortex.

The unique high affinity uptake systems for glutamic and aspartic acids and glycine in central nervous tissue constitute neurochemical evidence that these compounds are transmitter substances. Such uptake systems may terminate their postulated synaptic effects. The occurrence of selective neuronal uptake systems for most putative neurotransmitters, with the possible exception of acetylcholine, suggests that neuronal re-uptake may be a major mechanism for terminating the synaptic activities of neurotransmitters.

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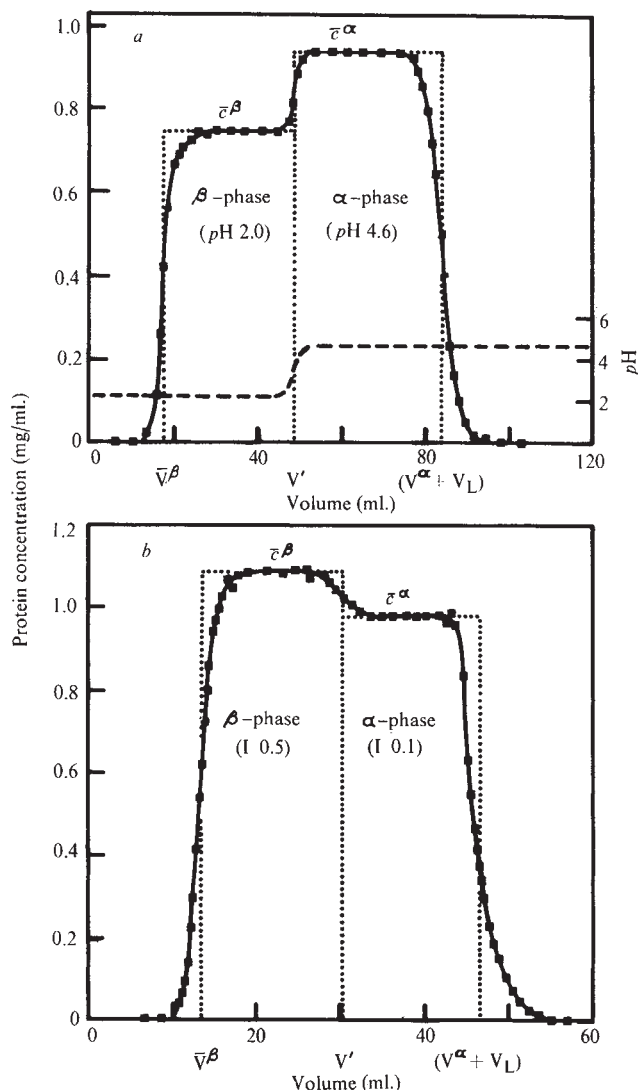
## Differential Chromatographic Study of Macromolecular Changes governed by Environmental Factors

GILBERT<sup>1</sup> developed a differential gel chromatographic technique to compare the extents of association of two different haemoglobin species. This entailed the layering of a relatively large volume of solution of one species onto a column equilibrated with an identical concentration of the other. Differences in the weight-average elution volumes of the two solutes resulted in a change of protein concentration at the junction of the two solutions, this effect being a consequence of mass conservation. We wish to explore Gilbert's suggestion<sup>1</sup> that the differential method could be adapted to study the effects of environmental factors ( $p\text{H}$  and ionic strength,  $I$ ) on the state of a single solute system. The acid expansion of bovine serum albumin provides an example where a variation in  $p\text{H}$  induces a change in conformation and thus in elution volume. The effect of ionic strength on the extent of association of  $\beta$ -lactoglobulin A has also been studied, the change of weight-average elution volume in this instance being used to compare the association constants pertinent to two environments of different ionic strength.

To a  $1.8 \times 19.6$  cm column of 'Sephadex G-100', thermostated at  $20^\circ\text{C}$  and equilibrated with 0.1  $I$  buffer,  $p\text{H}$  2 (0.01 M HCl, 0.09 M NaCl) was added 60 ml. of bovine serum albumin (0.94 mg/ml.) dissolved in 0.1  $I$  buffer,  $p\text{H}$  4.6 (0.01 M sodium acetate, 0.01 M acetic acid, 0.09 M NaCl). The eluate from the column was monitored spectrophotometrically at 280 nm and the volume of fractions was determined accurately by weight. The same method was used to obtain the elution profile in an experiment wherein 35.1 ml. of  $\beta$ -lactoglobulin A (9.81 mg/ml.) in 0.1  $I$  buffer,  $p\text{H}$  4.65 (0.1 M sodium acetate, 0.09 M acetic acid) was applied to a  $1.3 \times 23.8$  cm column of 'Sephadex G-75', thermostated at  $4^\circ\text{C}$  and equilibrated with 0.5  $I$  buffer of the same  $p\text{H}$  (0.1 M sodium acetate, 0.09 M acetic acid, 0.4 M NaCl).

The results obtained in the experiment with bovine serum albumin are shown in Fig. 1a from which it is evident that the profile of protein concentration (—) has two distinct plateau regions, the initial elution of protein being at a con-





**Fig. 1** Elution profiles resulting from (a) application of 60 ml. of bovine serum albumin (0.94 mg/ml.) in acetate-chloride buffer (pH 4.6) to a 50-ml. column of 'Sephadex G-100' equilibrated with pH 2 buffer, and (b) application of 35.1 ml. of  $\beta$ -lactoglobulin A (9.81 mg/ml.) in 0.1 I acetate (pH 4.65) to a 30-ml. column of 'Sephadex G-75' equilibrated with 0.5 I acetate-chloride buffer (pH 4.65). Temperature: a, 20° C; b, 4° C. Flow rate of each column: 30 ml./h.

centration less than that applied ( $\bar{c}^\alpha$ ). The concentration gradient separating the two plateaux coincides with the elution of the proton gradient measured separately and shown by the broken line in Fig. 1a (relevant ordinate scale on right). It is noted that the experimental procedure differs from that used by Gilbert<sup>1</sup> inasmuch that prior equilibration of the column with protein is not required: in the present design, the plateau of protein in the pH 2 medium is created during the experiment, because of its faster migration rate compared with that of the hydrogen ions. The difference between the two observed plateau concentrations reflects the faster migration rate (smaller elution volume) of bovine serum albumin in the more acidic environment where unfolding has occurred<sup>2</sup>. In the experiment conducted with  $\beta$ -lactoglobulin A (Fig. 1b) two plateaux are also observed, but in this case the initial plateau exceeds the applied concentration. This implies that the protein acquires a smaller velocity (larger weight-average elution volume) on passing through the junction between the environments of different ionic strength. Both results are consistent with the following geometric interpretation. The dotted lines in Fig. 1 are median bisectors of observed concentration gradients and permit representation of irregular outlines as rectangles of equivalent area. The total amount of solute added to the

column is  $\bar{c}^\alpha V_L$ , where  $V_L$  is the loading volume, and for mass to be conserved equals the sum of the areas of the rectangles shown in Fig. 1. Thus

$$\bar{c}^\alpha V_L = (V' - \bar{V}^\beta) \bar{c}^\beta + (\bar{V}^\alpha + V_L - V') \bar{c}^\alpha \quad (1)$$

which on rearrangement yields

$$\frac{\bar{c}^\alpha}{\bar{c}^\beta} = \frac{V' - \bar{V}^\beta}{V' - \bar{V}^\alpha} \quad (2)$$

where bar superscripts denote constituent quantities<sup>3</sup>. Equation (2) is the Johnston-Ogston equation<sup>4</sup>, with  $V'$  identified as the elution volume of the median bisector of the protein constituent gradient<sup>5</sup>.

In relation to Fig. 1a, the following values of the parameters in equation (2) pertain:  $\bar{c}^\alpha = 0.94$  mg/ml.,  $V' = 48.8$  ml.,  $\bar{V}^\beta = 17.7$  ml.,  $\bar{V}^\alpha = 24.0$  ml. Their substitution in equation (2) yields a value of 0.75 mg/ml. for  $\bar{c}^\beta$ , in complete agreement with that observed experimentally. In a reverse situation where the column was equilibrated with pH 4.6 buffer and bovine serum albumin loaded in a pH 2 environment, the condition  $\bar{V}^\alpha < \bar{V}^\beta$  pertained and it was observed that  $\bar{c}^\beta > \bar{c}^\alpha$ , the result again being in quantitative accord with equation (2). In connexion with the result obtained with  $\beta$ -lactoglobulin A, summarized in Fig. 1b, excellent agreement was again found between observed elution volumes (estimated by first moment calculations<sup>6</sup>) and those predicted on the basis of equation (2), the values being  $\bar{V}^\beta = 13.5$  ml. and  $\bar{V}^\alpha = 11.5$  ml. In pH 4.65, 0.1 I buffer at 4° C,  $\beta$ -lactoglobulin A seems to exist in solution as an equilibrium mixture of essentially monomer, A (molecular weight 36,000), and tetramer, C (molecular weight 144,000), characterized by an association constant,  $K$ , of  $1.08 \times 10^{-2} \text{ l.}^3 \text{ g}^{-3}$  (compare ref. 7, which cites earlier references). The definitions of the equilibrium constant and weight-average elution yield the following simultaneous equations.

$$K(c_A^\alpha)^4 + c_A^\alpha - \bar{c}^\alpha = 0 \quad (3)$$

$$\bar{V}^\alpha \bar{c}^\alpha = V_A c_A^\alpha + V_C (\bar{c}^\alpha - c_A^\alpha) \quad (4)$$

where  $c_A^\alpha$  is the equilibrium concentration of monomer in the  $\alpha$ -phase and  $V_A$  and  $V_C$  are the elution volumes of the monomer and tetramer species, respectively. With  $V_C$  taken as the void volume of the column (9.3 ml.), equations (3) and (4) were solved for the remaining unknown,  $V_A = 13.9$  ml. Equations (3) and (4) may be written for the  $\beta$ -phase, whereupon  $c_A^\beta$  and  $K$  for the 0.5 I environment may be calculated, provided it is assumed that the individual values of  $V_A$  and  $V_C$  pertain to both  $\alpha$  and  $\beta$  phases. A value of  $1.0 \times 10^{-4} \text{ l.}^3 \text{ g}^{-3}$  was found for  $K$  in the 0.5 I medium, which clearly shows that an increase of ionic strength promotes the dissociation of tetrameric  $\beta$ -lactoglobulin A.

In summary, the two systems studied illustrate the potential of the differential method for detecting changes in the shape or extent of association of a single solute consequent upon a suitable change of environment. The major advantage of the method is that the change is detected not only by the difference between the observed elution volumes in the two environments ( $\bar{V}^\alpha$  and  $\bar{V}^\beta$ ), but also by the existence of two plateau regions separated by a concentration gradient, the direction of which specifies immediately the relative magnitudes of  $\bar{V}^\alpha$  and  $\bar{V}^\beta$ . The method should be of particular value for the study, in a wide range of environments, of polymerizing systems which have been previously characterized by a variety of methods under one set of environmental conditions.

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## NAD in the Metabolism of Motile Spermatozoa

THE difficulty in attempting to correlate the levels of nicotinamide nucleotide coenzymes with the metabolism of intact animal cells is to distinguish clearly between changes in the coenzyme oxidation-reduction levels which arise from the metabolism of "resting" cells and changes which are associated with other types of cellular activity, such as growth, division and multiplication. Other problems are how to maintain cell viability and what parameters to use in assessing that viability. Spermatozoa are therefore particularly convenient as experimental objects, for they neither grow nor multiply, but can survive for a long time inside and outside the body. Their viability can be checked by quantitative measurements of motility. Furthermore, at least in respect of carbohydrate metabolism, mammalian spermatozoa depend for their supply of energy on extracellular sources, chiefly in the form of fructose which they utilize anaerobically and aerobically, at a rate which is directly and closely related to the degree of sperm motility<sup>1-3</sup>. Thus, changes in the concentration of intracellular coenzymes during fructolysis are directly attributable to carbohydrate metabolism.

The chief end-product of fructolysis in semen is lactic acid, but there are also small amounts of other carbohydrate metabolites, including sorbitol which is oxidized by the intracellular sorbitol dehydrogenase (ketose reductase, L-iditol:NAD oxidoreductase) of spermatozoa<sup>4</sup>, according to the equation



The chemical equilibrium of the reaction catalysed by the purified sorbitol dehydrogenase *in vitro* favours the reduction of fructose to sorbitol rather than the reverse, yet in washed but intact and motile ram spermatozoa the added sorbitol is oxidized faster than fructose is reduced. This has been attributed to the ratio of NAD<sup>+</sup> to NADH in intact spermatozoa.

We have therefore determined (i) the steady state levels of the oxidized and reduced NAD within spermatozoa and (ii) the direction of changes in the levels of total intracellular NAD<sup>+</sup> and NADH within actively metabolizing sperm cells, under the influence of various extracellular substrates. Our

procedure for NAD<sup>+</sup> and NADH determination is based on that of Greenbaum *et al.*<sup>5</sup>, which involves measuring the rate of oxygen uptake with an oxygen electrode with the extracted NAD<sup>+</sup> and NADH as rate limiting carriers for hydrogen transfer reactions catalysed by alcohol dehydrogenase, and phenazine methosulphate as electron acceptor.

Washed sperm suspensions were prepared from ram or boar semen by washing and resuspending spermatozoa in Ringer-phosphate<sup>3</sup>, and making up to a final concentration of 0.5–2 × 10<sup>9</sup> sperm/ml. The changes in the concentration of total intracellular NAD<sup>+</sup> and NADH, resulting from the addition of various substrates to sperm suspensions, were determined after incubation for 1–60 min, anaerobically, at 25° C.

The results of coenzyme determinations in ram and boar semen (the latter collected as the sperm-rich fraction) and in washed sperm suspensions are shown in Table 1. Our values for the sum of NAD<sup>+</sup> and NADH in ram sperm were not very different from those reported by Bistocchi *et al.*<sup>6</sup> for bull epididymal spermatozoa. In boar spermatozoa we found less than half the amount of NAD<sup>+</sup>+NADH than is present in ram sperm. In both species the values for NAD<sup>+</sup> were similar to those recently reported by Brooks<sup>7</sup>. Washing or incubating the washed spermatozoa produced no significant change in the sum of intracellular NAD<sup>+</sup> and NADH, but the ratio between the oxidized and reduced form of the coenzyme was significantly altered by the washing procedure; in the ram sperm it declined ( $P < 0.001$ ), while in boar sperm it increased ( $P < 0.05$ ).

We next investigated exogenous substances for ability to induce changes in the NAD<sup>+</sup>/NADH ratio in incubated spermatozoa, particularly those metabolites which were known to be readily metabolized by living spermatozoa and are known, from experiments on purified, reconstituted enzyme-coenzyme systems, to require soluble, NAD-linked dehydrogenases for their oxidation or reduction *in vitro*. All substances which changed the oxidation state of NAD in the living sperm cells were identical to those known from enzymological studies to depend on the availability of NAD<sup>+</sup> or NADH for oxido-reductions and energy generation. Up to a point, we could predict from the sperm coenzyme response whether or not the sperm cells possess a dehydrogenase capable of reacting with a particular substance. For example, in washed ram spermatozoa the addition of sorbitol produced within 1 min a reduction of sperm-NAD<sup>+</sup> to NADH, whereas only a slight reduction occurred when boar spermatozoa were used instead, even on extended incubation. This suggested that boar spermatozoa are deficient in sorbitol dehydrogenase. When we tried to extract the soluble NAD-linked sorbitol dehydrogenase from ram and boar spermatozoa<sup>8</sup>, we obtained a highly active preparation from ram spermatozoa, but were unable to detect the enzyme in boar sperm extracts. Similarly, we could show that of the two isomers of lactic acid, only L(+), but not D(–) lactate, can influence the oxidation-reduction state of NAD within spermatozoa, in accordance with what is known about the response of spermatozoa to lactic acid<sup>9,10</sup>, and the selective specificity of lactate dehydro-

**Table 1** Oxidized and Reduced Forms of NAD in Semen and Washed Suspensions of Motile Spermatozoa

|      |              | Number of samples | NAD <sup>+</sup> | NADH      | NAD <sup>+</sup> + NADH | $\frac{\text{NAD}^+}{(\text{NAD}^+ + \text{NADH})} \times 100$ |
|------|--------------|-------------------|------------------|-----------|-------------------------|----------------------------------------------------------------|
|      |              |                   |                  |           |                         |                                                                |
| Ram  | Semen        | 7                 | 19.1 (0.7)       | 5.5 (0.3) | 24.6 (0.8)              | 77.6 (1.8)                                                     |
|      | Washed sperm | 9                 | 14.7 (0.8)       | 9.3 (0.6) | 24.0 (1.2)              | 61.2 (1.3)                                                     |
| Boar | Semen        | 6                 | 7.5 (1.0)        | 1.4 (0.4) | 9.0 (0.9)               | 83.5 (5.3)                                                     |
|      | Washed sperm | 6                 | 9.2 (1.2)        | 0.3 (0.1) | 9.4 (1.2)               | 96.6 (0.9)                                                     |

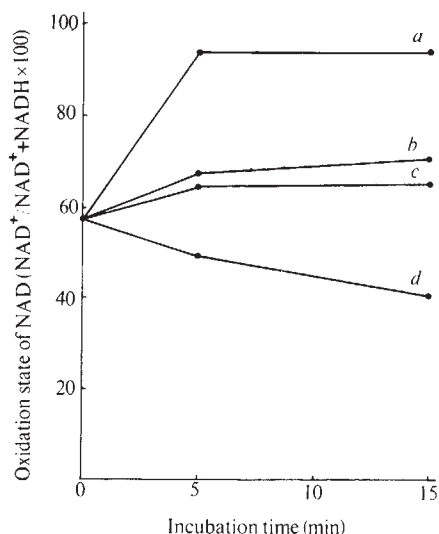
Results are expressed in nmol/10<sup>9</sup> sperm; standard error of the mean in parentheses.



genase for the L(+) isomer. As could be predicted from the nature of the reaction catalysed by lactate dehydrogenase (L-lactate : NAD oxidoreductase)



the addition of lactate to washed suspensions of either ram or boar spermatozoa produced a rapid conversion of  $\text{NAD}^+$  to NADH within the sperm cells, while pyruvate produced the opposite effect. The effect of lactate and pyruvate on the oxidation state of NAD in washed ram spermatozoa is illustrated in Fig. 1, which also includes the result obtained with fructose. The addition of fructose to washed ram spermatozoa produced a small increase in the percentage of  $\text{NAD}^+$ , probably because of a small initial reduction of fructose to sorbitol by ram sperm sorbitol dehydrogenase. When this experiment was repeated with washed boar spermatozoa which, as stated above, are deficient in sorbitol dehydrogenase, the effect of added fructose was that of an immediate decrease in the percentage of  $\text{NAD}^+$ .



**Fig. 1** Effect of fructose, lactate and pyruvate on the oxidation state of NAD in washed ram spermatozoa. The mixtures contained  $1.7 \times 10^9$  sperm and 2 mg substrate/1 ml., and were incubated at 25° C. a, Pyruvate; b, fructose; c, no addition; d, lactate.

Among other exogenous substrates which we have tested, three deserve special mention because of their strong oxidizing influence on sperm-NADH, namely, dihydroxyacetone, oxaloacetic acid and acetoacetic acid. All three substances are known, from previous studies of sperm metabolism, to be metabolized efficiently by motile spermatozoa<sup>3,11</sup>.

The influence of nicotinamide nucleotide coenzymes on cell metabolism is well documented<sup>12</sup>. The observations reported here, however, provide insight into the controlling influence of extracellular metabolites on the oxidation-reduction state of NAD within the cells. In motile, actively metabolizing spermatozoa, the ratio between total  $\text{NAD}^+$  and total NADH is regulated in a rapid and highly sensitive manner by energy yielding processes originating in the extracellular environment. Our findings also underline the important role allotted to exogenous substrates secreted by the reproductive tract, be it the seminal plasma of the male or the secretory fluids of the female, in regulating the metabolic behaviour of spermatozoa, in respect, at any rate, of those enzymatic reactions that are mediated by NAD-dependent sperm-dehydrogenases. At the same time, the determinations of the oxidized and reduced forms of NAD in spermatozoa provide a means for rapid

recognition of extracellular metabolites potentially capable of undergoing oxidoreductive breakdown by spermatozoa.

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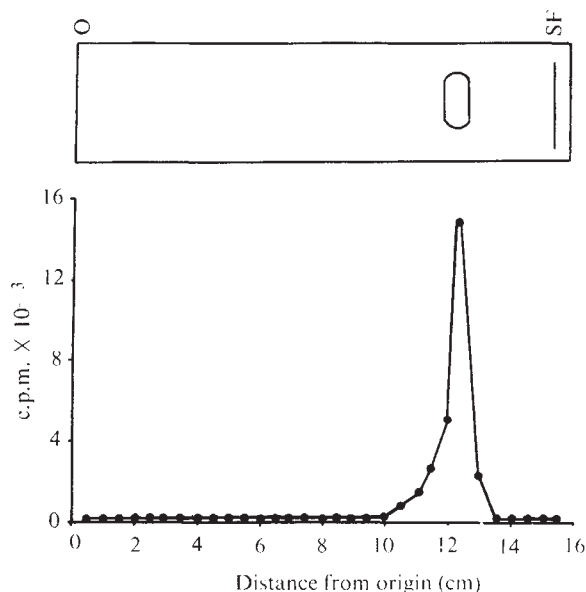
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## Autoradiographic Localization of $^3\text{H}$ - $\beta$ -Ecdysone in Salivary Gland Cells of *Drosophila virilis*

ALTHOUGH the hormone ecdysone is known to initiate moulting and metamorphosis in insects its precise mechanism and site of action are unclear. When injected into larvae of *Chironomus tentans* it stimulated the formation of specific puffs in the polytene, salivary gland chromosomes<sup>1</sup>, and two mechanisms have been proposed to explain this. Karlson and Sekeris<sup>2</sup> postulated that the hormone acts directly on the genome to "derepress" it, allowing the DNA to be transcribed. Kroeger<sup>3-5</sup>, however, suggested that ecdysone activates a



**Fig. 1** Thin-layer chromatographic separation of authentic  $\beta$ -ecdysone (upper part of figure) and radiochromatogram of chloroform and ethanol extract of tissue incubated with  $^3\text{H}$ - $\beta$ -ecdysone for 30 min (lower part of figure). Silica gel HF 254 + 366 is the solid phase and ethanol : chloroform : water (70 : 20 : 2) is the solvent. O, Origin; SF, solvent front.

sodium pump at the cell membrane. We have now investigated by autoradiography the distribution of  $^3\text{H}$ - $\beta$ -ecdysone in isolated salivary gland cells of *D. virilis*.

Salivary glands of *Drosophila virilis* were dissected from third instar larvae and incubated for 15–30 min in Ephrussi–Beadle medium<sup>6</sup> containing 10  $\mu\text{Ci}$  (1.6  $\mu\text{g}$ ) of  $^3\text{H}$ - $\beta$ -ecdysone (specific activity 3 Ci/mmol, New England Nuclear Corp.) or  $^3\text{H}$ -progesterone (specific activity 10 Ci/mmol, New England Nuclear Corp.). After incubation, glands were washed in Ephrussi–Beadle medium, transferred to gelatinized slides and stained with lactic-acetic-orcein for 3–5 min. The glands were squashed under a siliconized cover slip, frozen between blocks of solid  $\text{CO}_2$  and the cover slip removed. The tissue was fixed by immersing the slides in 10% formalin for 60 min. Kodak NTB-3 nuclear tracking emulsion was applied and the slides were stored in light-tight boxes for 40 to 70 days at 4° C. Autoradiographs were developed in Kodak D-19 developer.

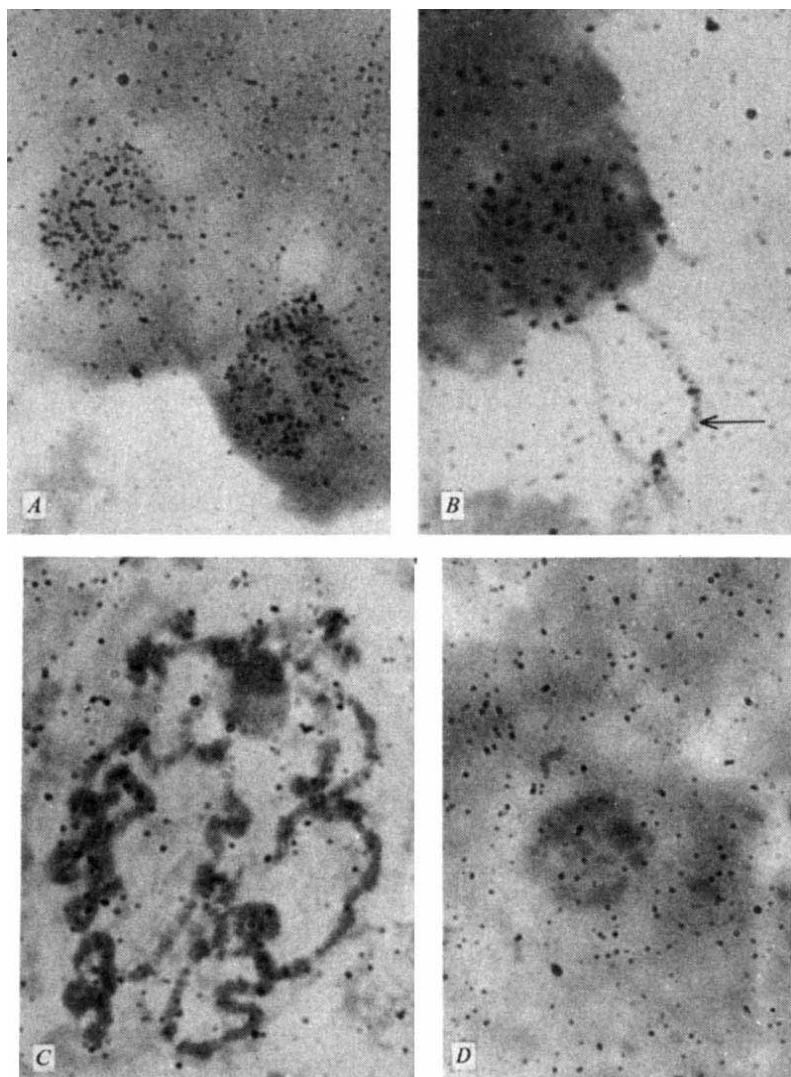
To determine whether the silver grains were due to  $\beta$ -ecdysone or a metabolite, the tissue was incubated with  $^3\text{H}$ - $\beta$ -ecdysone for 30 min, homogenized and extracted with chloroform and ethanol. The extract was analysed by thin-layer chromatography (TLC) using silica gel HF 254+366 as the solid phase and ethanol:chloroform:water (70:20:2) as the solvent. The distribution of radioactivity was determined by scanning the TLC plate in a Baird Atomic radiochromatogram scanner, model RSC-363. In further experiments to detect any possible metabolites of ecdysone 0.5 cm sections were scraped from the plates, eluted with ethanol, and counted in toluene scintillation fluid. The radioactivity of the chloroform and ethanol extract

(Fig. 1) co-chromatographed exactly with a sample of authentic  $\beta$ -ecdysone (Mann Research Laboratories). Thus there is no evidence that the silver grains were due to a metabolite of  $\beta$ -ecdysone.

Autoradiographs prepared from salivary glands incubated with  $^3\text{H}$ - $\beta$ -ecdysone showed that the radioactivity was concentrated over the nuclei and associated with the chromosomes (Fig. 2). In Fig. 2A the grains are arranged in a line over some of the chromosomes but are not concentrated at any particular gene loci. To test whether this was a specific concentration of the hormone, rather than nonspecific steroid binding, other salivary glands were incubated with  $^3\text{H}$ -progesterone. There was no concentration of radioactivity from this steroid hormone over any area of the cell (Fig. 2D). These results tend to support the hypothesis that ecdysone acts in the nucleus but do not completely rule out the model proposed by Kroeger since silver grains are evident over other areas of the cells although less concentrated.

$\alpha$ -Ecdysone, first isolated from silkworm pupae<sup>7</sup>, is thought to be the active moulting hormone in insects. The  $\alpha$ -ecdysone analogue  $\beta$ -ecdysone (20-hydroxyecdysone, ecdysterone, crustecdysone) can initiate metamorphosis of wing disks of *Galleria mellonella*, stimulate evagination of imaginal disks of *D. melanogaster* cultured *in vitro*, induce puffs in salivary gland chromosomes and stimulate protein and RNA synthesis in various insects<sup>8–12</sup>.  $\beta$ -Ecdysone has also been reported to be significantly more active than  $\alpha$ -ecdysone in inducing puparium formation in ligatured larval abdomens of *Diptera*<sup>13,14</sup>. Horn *et al.*<sup>15</sup> suggested that  $\alpha$ -ecdysone is the

**Fig. 2** Autoradiograms of squash preparations of *D. virilis* salivary glands. Glands were incubated *in vitro* with tritiated  $\beta$ -ecdysone (A, B, C) or progesterone (D). A, Silver grains are concentrated over two adjacent nuclei and some tend to parallel the underlying chromosomes ( $\times c. 600$ ). B, Note part of a chromosome protruding from the nucleus (arrow) ( $\times c. 840$ ). C, Silver grains associated with chromosomes ( $\times c. 990$ ). D, Note lack of concentration of radioactivity from progesterone over any area of the cell ( $\times c. 600$ ).





precursor of  $\beta$ -ecdysone, and this has recently been verified<sup>16,17</sup> for crustaceans and insects, which convert  $\alpha$ -ecdysone to  $\beta$ -ecdysone within 15 min. They suggested that  $\alpha$ -ecdysone is a very short lived intermediate in the synthesis of  $\beta$ -ecdysone.

Which ecdysone (if not both) is the active hormone in these insects? Our data showing that  $\beta$ -ecdysone is concentrated by nuclei of salivary gland cells and those reported by others suggest that  $\alpha$ -ecdysone is converted to the more active  $\beta$ -ecdysone just as testosterone is converted to a more active metabolite, 5- $\alpha$ -dihydrotestosterone, in its target tissue<sup>18-21</sup>. Support for this hypothesis comes from the work of Thomson *et al.*<sup>22</sup>, who found that  $\beta$ -ecdysone is also concentrated by the nuclei of the fat body of *Calliphora stygia*. Conflicting reports<sup>23,24</sup> make it unclear whether  $\alpha$ -ecdysone is also concentrated by salivary gland nuclei.

Emmerich<sup>25</sup> has recently reported that proteins in salivary glands of *D. hydei* bind  $\alpha$ -ecdysone. Our attempts to identify by standard techniques<sup>26-29</sup> specific cytoplasmic or nuclear "receptor" proteins in salivary glands of *D. virilis* which bind  $\beta$ -ecdysone have been unsuccessful. The mechanism by which  $\beta$ -ecdysone is concentrated by these nuclei remains to be determined.

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## Effect of Prostaglandin E<sub>1</sub> on Adjuvant Arthritis

We have found that when rats are treated with the prostaglandin PGE<sub>1</sub>, adjuvant arthritis is prevented and suppressed. Although the treatment had no effect on delayed hypersensitivity to PPD, it reduced the immune response to sheep red blood cells (SRBC). Our findings support earlier indications that PGE<sub>1</sub> might modify the inflammatory response<sup>1,2</sup>.

Adjuvant arthritis appears in rats 10-14 days after a single intradermal injection of Freund's complete adjuvant<sup>3</sup> and provides a convenient experimental model for *in vivo* evaluation of anti-inflammatory and/or immunosuppressive agents<sup>4</sup>.

Adjuvant disease was induced in adult male Holtzman rats by intradermal injection into the left hind paw of 250  $\mu$ g of steam-killed *Mycobacterium tuberculosis* in 0.05 ml. of heavy mineral oil. Three groups of rats received either 500  $\mu$ g of PGE<sub>1</sub>, 500  $\mu$ g of prostaglandin A<sub>2</sub> (PGA<sub>2</sub>) or buffer alone (0.06 M sodium phosphate buffer used to dissolve prostaglandins) subcutaneously twice daily for 21 consecutive days from the time of adjuvant injection (day 0). (Prostaglandins were a gift from Dr John Pike, Upjohn Company, Kalamazoo). Three groups received a similar 21 day course of PGE<sub>1</sub> beginning 7, 14 and 21 days after adjuvant injection. Severity of arthritis in the three uninjected paws was evaluated as described earlier<sup>4</sup>.

The most pronounced side effect of prostaglandin therapy was somnolence within 5 min of injection and lasting about 2 h. Similar effects on the central nervous system have been noted in rats after administration of comparable doses of PGE<sub>2</sub><sup>1</sup>. Transient diarrhoea was observed sporadically. Diffuse alopecia, apparently unrelated to injection sites, developed in 5 rats treated from days 7 to 27.

Severe polyarthritis developed in all control and PGA<sub>2</sub>-treated rats. Those treated with PGE<sub>1</sub> from day 0 and day 7 developed little or no arthritis; when treatment began on day 14 the typical explosive course of the arthritis, which had already

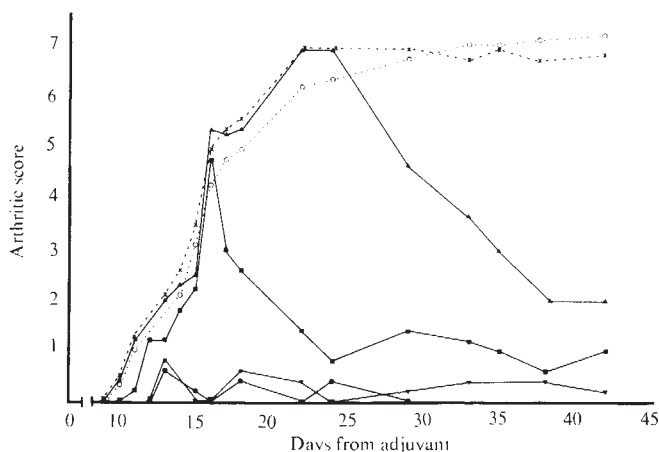
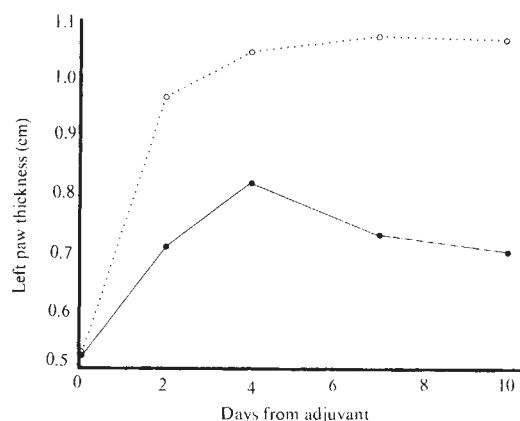


Fig. 1 Effect of prostaglandin on adjuvant arthritis. Each point represents the mean arthritic score for the three uninjected paws (five rats per group). Numbers in parentheses indicate period of treatment. x, Controls; ●, PGE<sub>1</sub> (0-20); ▼, PGE<sub>1</sub> (7-27); ■, PGE<sub>1</sub> (14-34); ▲, PGE<sub>1</sub> (21-41); ○, PGA<sub>2</sub> (0-27).



**Fig. 2** Effect of prostaglandin on adjuvant arthritis. Each point represents the mean thickness of the adjuvant injected paw (five rats per group treated from day 0). ○, Controls; ●, PGE<sub>1</sub>.

started, was suppressed. Established arthritis was considerably reduced even when treatment began on day 21 (Fig. 1). Arthritis did not develop in rats observed for 28 days after PGE<sub>1</sub> injections were stopped.

Rats treated with PGE<sub>1</sub> had a less intense inflammatory reaction at the site of adjuvant injection (Fig. 2), were more active and did not lose weight, nor develop the leucocytosis and anaemia characteristic of the disease and seen in the other two groups. The number of circulating lymphocytes was not diminished significantly by PGE<sub>1</sub>.

On day 14, control and PGE<sub>1</sub> treated rats were tested with 10 µg of PPD, injected into the skin of the right ear. Buffer alone was injected into the skin of the left ear. Ear thickness was measured with a direct reading micrometer at 24 h intervals for 3 days. The delayed hypersensitivity reaction was positive in all rats, including those completely protected from arthritis. Erythema and induration were greater and more persistent in treated than in control rats (Table 1).

**Table 1** Effect of PGE<sub>1</sub> on Delayed Hypersensitivity to PPD and Antibody Response to SRBC\*

|                  | Hours after PPD injection |     |     | Anti-SRBC titres |
|------------------|---------------------------|-----|-----|------------------|
|                  | 24                        | 48  | 72  |                  |
| PGE <sub>1</sub> | 108†                      | 210 | 189 | 8.72 ± 0.49‡     |
| Control          | 177                       | 69  | 41  | 11.92 ± 0.49     |

\* Control and PGE<sub>1</sub>-treated rats injected with 10 µg PPD into skin of ear, and 1 ml. 10% suspension SRBC intraperitoneally.

† Mean increase in thickness of the PPD injected ear minus thickness of buffer injected ear (five rats per group).

‡ Mean log<sub>2</sub> titre for each group of five rats 7 days after immunization, ± s.d.;  $P < 0.0005$ .

Control and treated rats were given 1 ml. of a 10% suspension washed sheep red blood cells intraperitoneally on day 14, and were bled 7 and 17 days later for anti-SRBC titres to be determined<sup>5</sup>. The brisk antibody response seen in controls was reduced markedly in rats treated with PGE<sub>1</sub> (Table 1).

The mechanism(s) by which PGE<sub>1</sub> suppresses adjuvant arthritis is unknown but its biological effects suggest several explanations. A stimulatory effect on the pituitary-adrenal axis is suggested by the ability of PGE<sub>1</sub> to increase steroidogenesis<sup>6-8</sup>. In rats treated with paramethasone, however, polyarthritis developed within 7 days after treatment was stopped<sup>9</sup>. Such an "escape" phenomenon was not seen after treatment with PGE<sub>1</sub>. There is experimental evidence that lysosomes can mediate tissue injury in joint disease<sup>10</sup>. The observation that PGE<sub>1</sub> reduces release of lysosomal enzymes from phagocytic cells *in vitro*<sup>2</sup> may be relevant to an anti-inflammatory activity *in vivo*. PGE<sub>1</sub> reduces uptake and

degradation of aggregated bovine serum albumin by mouse macrophages<sup>11</sup>. It is possible that in the rat PGE<sub>1</sub> interferes with uptake and/or processing of antigen by macrophages. Anti-macrophage globulin suppresses adjuvant disease<sup>12</sup> but arthritis appears within 2 days of termination of treatment. Arthritis did not appear after treatment with PGE<sub>1</sub> stopped, suggesting that an effect of PGE<sub>1</sub> on macrophages would be more permanent than simple inactivation of mature macrophages. PGE<sub>1</sub> inhibits phytohaemagglutinin-induced transformation of lymphocytes *in vitro* (unpublished results of R. Hirschhorn), but another thymus-dependent function, delayed hypersensitivity to PPD, was not suppressed in treated rats. The reduction in the anti-SRBC titre suggests that an effect of PGE<sub>1</sub> on a restricted population of lymphocytes might contribute to suppression of adjuvant disease.

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## Underwater and Aerial Visual Acuity in the Asian "Clawless" Otter (*Amblonyx cineria cineria*)

Gentry and Peterson<sup>1</sup> have compared data obtained from a sea otter (*Enhydra lutris*) on an underwater size discrimination task with data obtained by Schusterman *et al.*<sup>2</sup> using two sea lions (*Zalophus californianus*). Although Gentry and Peterson did not test the sea otter's vision in air, they concluded that its performance was inferior to that of the sea lion because the "pinniped eye" is emmetropic underwater with adaptations for aerial vision and that the "otter eye" (*Lutra*) was emmetropic in air with adaptations for underwater vision<sup>3</sup>. Such an interpretation of the difference between otters and sea lions emphasizes different dioptric mechanisms and ignores neural structure and organization at the retinal level, as well as at the level of the cortical projection areas.

Without determining that it can see better in air than underwater, the finding that the underwater visual acuity of the sea otter is poorer than that of the sea lion may have little connexion with their accommodative processes which are totally different<sup>3</sup>. Instead, the superior underwater visual acuity of the sea lion can be attributed to the superior resolving power of its retina. Although there has been no comparison of the detailed retinal structure and organization in otters and sea lions, the ratio of



receptor to ganglion cells in sea lion retinas<sup>4</sup> is consistent with the finding that sea lions can resolve grating spacings subtending a visual angle of 5.5 minutes of arc both in air and underwater<sup>5</sup>. Tests of the behaviour of the ferret (*Mustela f. renata* and *Mustela corninea*), a close relative of the otter, indicate that maximum visual acuity of these mustelids<sup>6</sup> in air is about one third that of the sea lion. Furthermore, the type of stimulus configurations initially used by Schusterman *et al.*<sup>2</sup> and subsequently by Gentry and Peterson<sup>1</sup> were relatively inappropriate for measuring visual acuity uncontaminated by intensity discrimination<sup>5</sup>. To resolve some aspects of these issues concerning the suitability of the visual systems of different aquatic mammals we tested the aerial and underwater visual acuity of two mature male Asian "clawless" otters (*Amblonyx cineria cineria*). These mustelids are widely distributed throughout south-east Asia. Their diet includes fish, frogs, crabs and birds, and they are reported to be used by the Malays for fishing<sup>7</sup>.

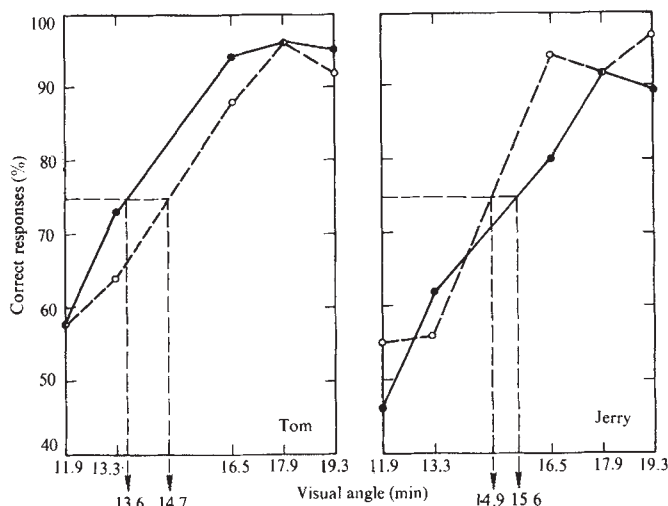


Fig. 1 The underwater (●) and aerial (○) visual acuity of otters (Tom and Jerry).

Both animals were trained and tested outdoors in a white plywood tank measuring  $2.3 \times 1.2 \times 1.6$  m. Detailed descriptions of the equipment and general procedures which were used are reported elsewhere<sup>8,9</sup>. Acuity targets were produced from 12.7 cm photos of Ronchi rulings with black and white stripes of equal width. The standard grating consisted of 300 lines per inch. The variable gratings consisted of lines varying in width from 12.7 mm to 4.8 mm. Targets were presented simultaneously by the experimenter lowering a target board from behind an opaque screen. The otters were maintained at a fixed distance of either 1.9 or 1.3 m from the targets with an opaque divider which projected down to the tank floor and outward from the opaque screen. The task was to approach the target with the variable grating in order to obtain a piece of chicken neck weighing 0.6 g. Once an animal left the start position after viewing the targets it was not allowed to retract. If, after leaving the start position, an otter swam or ran to the side of the divider with the standard target it had to push that target; otherwise, the targets were withdrawn and the response was counted as an "error". The minimum distance between the gratings and the animal was taken as 1.9 or 1.3 m. Training and determination of a range of acuity targets were accomplished in a manner similar to that described for the testing of pinniped visual acuity<sup>5,8,9</sup>.

Threshold estimates were obtained by the method of constants first used at a distance of 1.9 m with three variable targets subtending visual angles of 11.9', 16.5' and 19.3' for 20 days and then at a distance of 1.3 m with variable targets subtending

visual angles of 13.3' and 16.5' for 12 days. Each variable target was paired randomly with the standard for ten consecutive trials. Pairings were repeated twice for a total of sixty trials per day. Testing in air and underwater alternated daily.

Fig. 1 shows both aerial and underwater psychophysical functions for each otter. Thresholds may be found by noting the visual angles associated with 75% correct responses. These functions clearly show that the aerial and underwater visual acuity of *Amblonyx* is about the same. Threshold estimates measured in minutes of visual angle over the 16 test days were 13.6 and 15.6 for aerial acuity and 14.7 and 14.9 for underwater acuity. These figures are similar to those obtained on the aerial visual acuity of other mustelids<sup>6</sup>.

These results suggest that it is not the totally different dioptric mechanisms (as has previously been suggested<sup>1</sup>) which account for the inferior underwater visual acuity of otters compared with sea lions and seals, but rather the poorer resolving power of the otter retina in combination with other neural elements including the cortical projection areas. Apparently, the dioptric mechanism of otters is such that it allows the eye to function normally in air without any special accommodative effort and is also capable of producing enough accommodation underwater<sup>3</sup> so as not to limit the visual acuity of the submerged otter.

Because it is supposed that the protopinnipeds were lacustrine and that they fed along the shores of streams, lakes or the sea, as do some modern otters, and because it has been suggested that the true seals may have descended from an otter-like ancestor<sup>10</sup>, it is of considerable interest in terms of the evolutionary history of sensory systems of modern pinnipeds to compare the visual acuity of some otters with that of seals and sea lions. In spite of the limited data which are currently available it seems that the visual acuity of some pinnipeds<sup>4,5,8,9</sup> resembles more closely that of cats<sup>4</sup> than it does otters.

This research was supported by the US National Science Foundation. Reprints may be obtained from R. J. S.

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## Formation of a Carcinogen in Human Skin irradiated with Ultraviolet Light

THE role of ultraviolet light and sunlight in the aetiology of certain types of human skin cancer is now well established<sup>1</sup>. The mechanism(s) by which light induces such changes, however, are not understood. One explanation<sup>2,3</sup> of the induction of skin cancers by ultraviolet light and sunlight is that steroids in the

skin are converted photochemically to carcinogenic materials, but failure to obtain such compounds by irradiation of naturally occurring sterols in biological systems has cast doubt upon the validity of such a hypothesis. Although Blum has argued against this hypothesis<sup>4</sup>, he has recently described the induction by ultraviolet radiation of a diffusible substance that mediates hyperplasia<sup>5</sup>. Here we provide evidence for the formation of a cholesterol-derived carcinogenic photoproduct in human skin.

**Table 1** Formation of Photo-oxidation Products in Skin

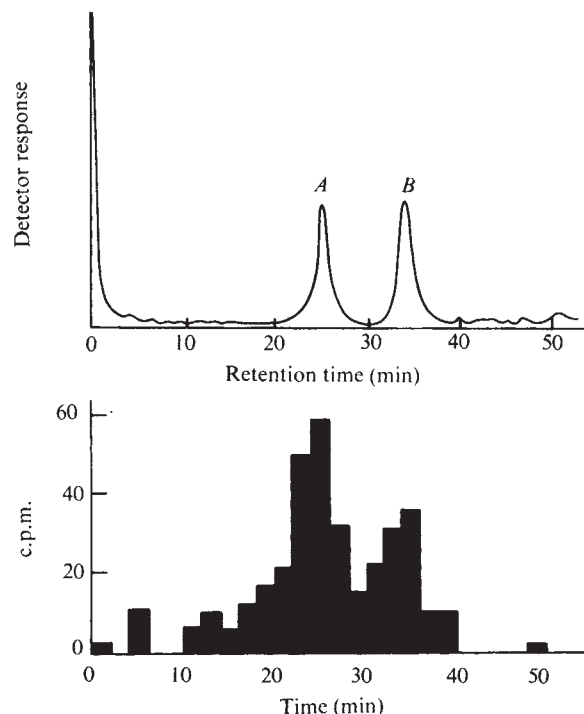
| Treatment        | Oxidation products* |
|------------------|---------------------|
| Control          | 1.04                |
| Irradiated (min) |                     |
| 2                | 1.43                |
| 10               | 1.84                |
| 20               | 3.68                |
| 60               | 9.20                |

\* Levels of polar photo-oxidation products were determined as the percentage radioactivity remaining at the origin of TLC plates, after development, from the total <sup>14</sup>C-cholesterol taken up by a unit weight of tissue. The yield of photo-oxidation products in the 60 min irradiated treatment is equivalent to approximately 26 ng/100 mg tissue. Note the relationship of photoproduct concentration to exposure time.

Skin obtained by surgery was immediately incubated in the presence of 5  $\mu$ C of <sup>14</sup>C-4-cholesterol (specific activity 61.7 mCi/mmol) suspended in Krebs-Ringer phosphate buffer (pH 7.4) for 18 h at 4° C. The specimens were then thoroughly rinsed and irradiated in an environmental chamber maintained at 37° C with an ambient air atmosphere. Irradiation was accomplished with a Burdick QA-450N mercury arc lamp for periods of 2, 10, 20, and 60 min ( $1.34 \times 10^5$  ergs/s/cm<sup>2</sup>). The chamber was fitted with a plane silica lens which transmitted all wavelengths emitted by the lamp (240–440 nm). Control specimens were maintained in identical conditions except that they were not irradiated.

After irradiation, the skin was cut into 50  $\mu$ m sections with a freezing stage microtome and the total lipid was extracted as described before<sup>6</sup>. The total lipid fraction was chromatographed on silica gel coated thin-layer chromatography (TLC) plates. They were developed in 1,2-dichloroethane and the polar material remaining at the origin containing the cholesterol-derived photoproducts was eluted. Experimental details and chromatographic characteristics of skin photoproducts in multiple solvent systems will be published later. The amount of photoproducts formed was estimated from the percentage of the total radioactive cholesterol taken up by the tissue (Table 1). The photoproducts were co-chromatographed (TLC) with several reference oxidation products of cholesterol and developed in a chloroform-acetone (9:1) system. Good separation was obtained for all reference compounds except 7-ketocholesterol and cholesterol  $\alpha$ -oxide which had the same  $R_F$  (0.83) values. The photoproduct fraction from skin migrating with the same  $R_F$  (0.83) as the two reference compounds was eluted and analysed by gas-liquid chromatography (GLC). A stream-splitter was placed in line with the flame ionization detector of the chromatograph. Cold reference compounds (7-ketocholesterol and cholesterol  $\alpha$ -oxide) were added to the skin photoproduct fraction and the effluent from the split stream of the gas chromatographic column collected at 2 min intervals. As shown in Fig. 1 the profile of the major areas of radioactivity of the photoproducts, as eluted from the GLC column, and the chromatographic detector response are in agreement. The data, together with the TLC characteristics of both 7-ketocholesterol and cholesterol  $\alpha$ -oxide, indicate that both compounds are present as constituents of ultraviolet-irradiated skin.

The mechanism involved in the formation of these photoproducts is unknown. One possible explanation is that photoproduct formation is the result of a primary photochemical oxidation. Alternatively photoproduct formation could be a result of the action of an intermediate oxidant on the sterol, that is, O<sub>3</sub>, singlet oxygen and so on.



**Fig. 1** Gas chromatographic record of (A) cholesterol  $\alpha$ -oxide and (B) 7-ketocholesterol. Cold reference compounds were added to the TLC fraction of skin photoproducts demonstrating the same  $R_F$  value as these references. The histogram represents counts per minute (c.p.m.) of <sup>14</sup>C in sequential fractions of the gas chromatographic effluent. These data were obtained with the 60 min irradiated skin sample. The cholesterol  $\alpha$ -oxide and 7-ketocholesterol fraction represented about 30% of the total photoproducts formed in skin.

Cholesterol  $\alpha$ -oxide has been shown to be carcinogenic in both rats and mice and it is the only oxidation product of cholesterol to have carcinogenic properties in the absence of an oily vehicle<sup>7</sup>. As far as we know, this is the first report of light-induced formation of a carcinogenic compound from naturally occurring sterols in human skin. Studies are in progress to examine the potential relationship of photo-oxidation product formation to light-induced skin cancer.

This work was supported in part by a Robert A. Welch Foundation grant.

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## Visual-Auditory Distance Constancy

WE wish to describe a new perceptual constancy phenomenon which may be termed auditory distance constancy. If an observer views an event which produces a sound it seems that some compensation is made for the fact that the event will stimulate the eye somewhat before it will stimulate the ear depending on the distance of the event from the observer. For example, the sound of an event 35 m away will arrive at the ear about 100 ms after the light arrives at the eye.

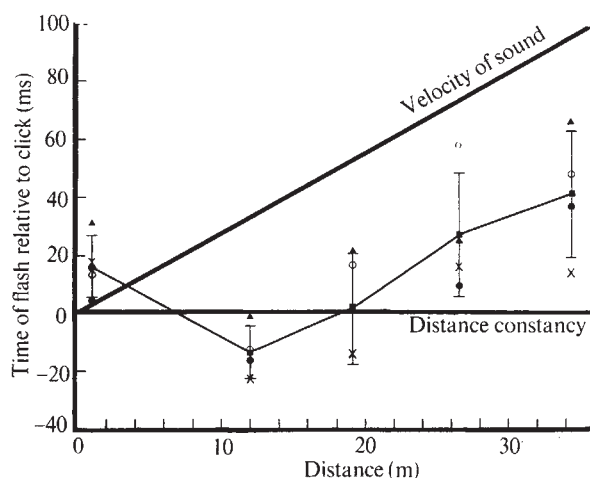


Fig. 1 Graph of the time delay of the flash relative to the click judged to represent a common event as a function of distance. Positive values represent the flash occurring after the click.  $\times$ ,  $S_1$ ;  $\bullet$ ,  $S_2$ ;  $\blacktriangle$ ,  $S_3$ ;  $\circ$ ,  $S_4$ ;  $\blacksquare$ , mean.

To investigate whether compensation is made for differences in arrival time of light and sound we asked observers to judge whether a light flash and a click were simultaneous as a function of distance. The light was a model PS2E Grass photo stimulator mounted in a box whose front was 47 cm wide and 36 cm high. As seen by the observer, the front of the box contained a white square ( $15 \times 5$  cm) on a black background. The photo stimulator produced a brief illumination of the white square. The light box was set on a laboratory stool which was partly covered with a black cloth used to conceal a small loudspeaker. A 0.5 ms pulse fed to the speaker produced the click. Timing of the flash and the click was the function of two American Electronics Laboratories stimulators.

For the experiment an observer was seated at one end of a long corridor and the light box and speaker were located at various distances down the corridor. The corridor gave good distance cues. The light flash and click were presented with various delays between the flash and the click. The delays of the flash relative to the click were -200, -150, -100, -50, 0, 50, 100, 150 and 200 ms. (A positive delay means the click occurred before the flash.) The observer was asked to adopt the attitude that the click and the flash would ordinarily be the same event. Thus for a given click-flash combination he had to decide whether the combination met the criterion of representing a single event. The distances used in the experi-

ment were 1, 11.9, 19.2, 26.5 and 34.4 m. Four observers were used in the experiment. For two observers each click-flash delay was presented ten times at each distance. For the other two observers each combination was presented fifteen times.

The results of the experiment are shown in the graph of Fig. 1. The ordinate represents the average delay of the flash relative to the click judged by each observer to represent a common event at each distance. The abscissa represents the distance at which the observation was made. A line is drawn through the mean delay for all four observers. Also shown is the standard deviation of the observers.

In addition to the observed data two theoretical lines are shown in Fig. 1. The horizontal line through zero delay represents the observations that would be obtained if the observers took perfect account of distance and the velocity of sound. The increasing straight line function represents observations that would be obtained if the observer took no account of distance and velocity but based their judgments on the simultaneous arrival of the sound and light at the observer. For this line the velocity of sound was taken as  $344.45 \text{ m s}^{-1}$  (calculated for dry air at  $22^\circ \text{C}$ ). By comparing the observations with the two theoretical lines it may be seen that the observers corrected for the velocity of sound for distances up to about 20 m beyond which the constancy begins to break down. Even at 35 m, however, there is still compensation by a factor of 0.5. As to the pattern of observations at the shorter distances it is difficult to say whether the apparent undercompensation at 1 m and overcompensation at 11.9 m is genuine or due to peculiarities of the situation.

A further observation worth noting on the graph is that the standard deviations of the observers' means at various distances are fairly small ranging from 9 to 22 ms. On the other hand, the standard deviation for individual observers at each point ranged from 50 to 100 ms. Thus the variability of the observers relative to one another was about half that which would be expected on the basis of the individual variability. Although judgments of perceived simultaneity are typically in the region of 50–100 ms, this would not explain the small between-observer variability. We believe that the reason for the discrepancy between individual and group variability is that the distribution of judgments for an individual at a given distance may be bimodal, one mode corresponding to simultaneity of the flash and click at the observer and one to simultaneity at the source. To establish this, however, would require more careful measurements and longer distances.

Because the experimental results implied that observers compensated only partially for longer distances, a size constancy experiment was carried out to get estimates of the perceived distances. That is, the partial compensation could have resulted from underestimating the distances involved. The estimates were obtained by having each observer adjust a variable size white square on a black background, viewed at a standard distance of 3 m, to match the absolute size of the white square in the light box as a function of the distance of the light box. The settings of the comparison square relative to the true size of the square on the light box were used to estimate the perceived distance of the light box. It was found that in general the perceived distances matched the true distances very well and that in no case would the observer's error in distance perception account for failure to compensate completely for the auditory time delay.

This work was supported by a National Research Council of Canada grant to G. R. E.

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# CORRESPONDENCE

## Environment Conference

SIR,—I should like you to know how much I appreciated the coverage of the Canberra meeting of SCOPE on September 1–3<sup>1</sup>. I must point out, though, that your correspondent entirely misunderstood the main thrust of my remarks on the question of the organization of environmental activities within the UN system. Far from advocating the creation of a “new agency”, I specifically pointed out that environmental affairs do not lend themselves to the creation of another new specialized agency because they involve the interaction of a number of cause and effect relationships which transcend the traditional boundaries of sectors and institutions. I emphasized that we are still at too early a stage in our consideration of this important question to know what decisions governments will take concerning it at Stockholm. In outlining my own views as to the present state of thinking by governments, I made two points: (1) that some form of continuing machinery will be needed at the international level after the Stockholm Conference to follow up the decisions taken at Stockholm, and (2) that what is required principally is a better means of coordinating and providing a central forum and a concerted sense of direction to the environmental activities of the existing international organizations, rather than a new specialized agency.

I have set out here a list of the criteria which should, in my view, as I said at Canberra, be applied in determining the kind of international organizational arrangements that may be made following the Stockholm Conference. As these have been accepted in principle by the Preparatory Committee of twenty-seven governments set up by the UN General Assembly, and I believe are fully compatible with the views of the UN agencies concerned, they represent a reasonable indication of present thinking on this important subject. (a) Any organizational arrangements should be based first on agreement

about what needs to be done. Until this is reached, no firm decision can be made on the ways and means to be adopted. (b) All functions that can best be performed by existing organizations should be assigned to those organizations, both international and national, most capable of carrying them out effectively. No unnecessary new machinery should be created. (c) It is more logical to consider a network of national, international, functional and sectoral organizations with appropriate linkages and “switchboard” mechanisms, whereby international organizations supplement and complement national organizations, than to think in terms of a global “super agency”. (d) Any action envisaged should allow for the preliminary state of knowledge and understanding of environmental problems, and should be flexible and evolutionary. (e) Governments will want to attach highest priority to the need for coordination and rationalization of the activities and programmes of the various international organizations active in the environmental field. This is essential in order to avoid overlap and duplication and to assure most effective use of scarce resources of money and manpower. (f) Any policy centre that is expected to influence and coordinate the activities of other agencies should not itself have operational functions which in any way compete with the organizations over which it expects to exercise such influence. (g) In the establishment of any additional or new machinery it is essential to provide strong capability at the regional level. (h) The United Nations should be the principal centre for international environmental cooperation. (i) The organization of environmental activities within the United Nations should be so designed as to strengthen and reinforce the entire United Nations system.

While there can be and no doubt will be differences of view both in the UN family and amongst governments concerning the specific decisions which must be taken at Stockholm in applying these criteria, the degree of consensus

that has already been achieved is a hopeful sign that these differences will be resolved successfully. The basic purpose of the preparatory process preceding Stockholm is precisely to enable all the relevant views to be fully discussed and reflected in the decisions governments will be asked to take at Stockholm.

Yours faithfully,

MAURICE F. STRONG

*Secretary-General  
of the Conference*

*United Nations Conference  
on the Human Environment,  
Stockholm*

<sup>1</sup>*Nature*, **233**, 81 (1971).

## Responsibility and the US National Academy

SIR,—In the November 5 issue of *Nature* (**234**, 7; 1971) there was a fairly extensive report of the reaction of the National Academy of Sciences to my resignation from that august body. In amplification of that report I would like to make a minor and a major point. The minor point is that I am *in fact* no longer a member of the National Academy, having ceased to be a member on the day of my resignation. Whether the Academy chooses or does not choose to accept my resignation is of no consequence. As I wrote recently to the President, “Membership in a voluntary association demands consent. I have withdrawn my consent”.

The major issue concerns the meaning of responsibility. Apparently reports of the NAS-NRC in the future will carry a disclaimer of responsibility by all members of the Academy except for those who may have actually served on the working committee. It is suggested by your correspondent that this goes some significant way toward meeting my objections to the Academy's secret research. No one who is not either politically naive or Machiavellian can subscribe to such a view. “Responsibility” means both literally and in practice that one is in a



position to be "called to account". Thus, responsibility inheres in a social relation and is defined by those who will do the calling, not by those who are called. It would be very convenient, of course, if each of us could define for himself when he was or was not responsible. Perhaps I could put a large sign on my car saying "This car is none too reliable and I will not be responsible for anyone killed by it". Mr Eichman said he only made up train schedules and was not responsible for what happened to people at the other end of the line. But Mr Eichman's victims and their families called him to account. It was they who defined the responsibility. The fact of responsibility cannot be waved away by wishful thinking. The facts are: (1) The National Research Council is the "operating arm of the National Academy". (2) The President of the Academy heads the National Research Council. (3) Officers of the National Academy have the power to determine policy of the National Research Council. (4) According to a letter from the President of the Academy, "The prestige of the National Academy of Sciences is used to recruit working Committees of the National Research Council". (5) The prestige of the National Academy stands behind every report and, indeed, every report has been reviewed by a special committee of the Academy.

Thus the Academy *is* responsible and no legal fiction of its own devising will change that fact. The best the members of the Academy can hope for is that accounts will not be called in their lifetime, or, if events move more swiftly, that the National Academy will be seen, in retrospect, as unimportant.

Yours faithfully,

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*University of Chicago*

## Fulfilling Fulton

SIR,—I was struck by the headline "Fulton Fulfilled" in your issue of October 8 (*Nature*, **233**, 365; 1971). I have only now seen it on my return from holiday, and I wondered what exciting civil service developments I had missed.

The introduction of an interim Science Category, satisfactory in structure terms but with pay arrangements that, despite our success in improving on the arbitration award, are still wholly unsatisfactory, is a long way indeed from the fulfilment of Fulton.

There is the question of pay. The Institution did not accept that pay research was valid for scientists, and we refuse now to believe that they can reasonably be paid less than technologists or administrators. We are determined that we shall not again have pay research applied in an area where it is

so clearly inapplicable. We shall redouble our efforts over the coming months to find a relevant basis for the pay of scientists. Then there is the question of careers; as I believe, the most important issue of all. On that, we are currently engaged in discussions with the Civil Service Department, and I am optimistic that we shall make real progress by the spring of next year.

Yours faithfully,

CYRIL COOPER

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## Population and Economics

SIR,—I suppose that your highly irresponsible editorial of November 5 (*Nature*, **234**, 2; 1971) under the heading "Alice Economics" need not be considered seriously, but there is a danger that what you have written will be read by "young people and other innocents", to use your own phrase in the other context. Professor Scorer, whose views you criticize, is not the only one to draw attention to the ecological predicament which will be speeded up by an expansion of EEC, and it is a pity that you can do no more than poke fun of him; indeed the fact that you do this indicates your own uneasiness.

You suggest that Professor Scorer's premises are false by taking a series of examples. Thus you ask if it is an accident that the richest nation on Earth is the one most concerned about pollution. No, it is not an accident, but must we industrialize and pollute every nation in the world before we become interested in the problems we are creating? You also ask if there is anything but economic growth that will free India of cholera. To free India of cholera would simply add a few more to the 14 or so million people that India is adding to the world every year. Economic growth cannot possibly help India and similar countries: if everyone in the world enjoyed the same standard of living and consumption as the average Londoner, the world's resources would disappear almost instantaneously. Indeed economic growth in the EEC and other industrial groupings will do nothing for India and the many other desperate nations except to encourage them to sell their dwindling raw materials even more rapidly to the expanding industrial complexes which so many see as a solution to all economic difficulties. Professor Scorer may be wrong as to why the resources of the sea are not as rich as they used to be. You think that over-fishing is the problem, he suggests pollution. You may both be

right, but the common denominator is expanding human numbers and, especially in Europe, expanding human needs.

To argue that entering the EEC is anything more than a conspiracy to create opportunities for big business is misleading. The expanded economic unit will demand more and more resources from the under-developed world, whose populations are likely to continue to increase at a phenomenal rate, thanks to the "aid" we offer as a small token of our appreciation for their help. You may think this a joke, but have you considered that, taking into consideration the resources it will consume, every child born in England, Sweden or America is forty or fifty times the ecological disaster of one born in Ghana, Sierra Leone or Liberia?

Yours faithfully,

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## Tax on Pollution

SIR,—The recently reported claim of the salvage industries to be given zero rating for value-added tax may not only attract wide support but can also be extended. The operation of VAT will automatically provide the machinery for a pollution-added tax, PAT, either as part of VAT or alongside it. By this means, the visible cost of each manufacturing process can gradually be made to include the social cost of the stress it imposes on the environment, and the price of a product can be made to cover the cost of its salvage, re-cycling or disposal.

Restrictive legislation, although a necessary adjunct, is widely recognized to be insufficient of itself to control environmental deterioration; it is too blunt an instrument, the enforcement of which is necessarily ponderous, expensive and therefore sporadic. PAT by contrast can provide effective economic pressures operating in detail day by day. Its application can be flexible, selective and finely graduated. Since rating for PAT would be subject to appeal based on expert testimony, it provides economic motivation for research aimed at elucidating the real effects of pollution and ecological disturbance. This knowledge is an essential prerequisite of right action in the preservation of our living environment.

Yours faithfully,

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# Announcements

## University News

**Dr W. G. Irwin** has been appointed to the chair of general practice, **Queen's University of Belfast**.

**Dr B. Benjamin**, Civil Service College, has been appointed visiting professor of applied statistics in the Graduate Business Centre, **City University**.

**Mr G. T. Goodman**, University College, Swansea, has been appointed to the chair of applied biology tenable at Chelsea College, **University of London**, and **Dr O. M. Wrong**, University of Dundee, has been appointed to the chair of medicine tenable at University College Hospital Medical School. The title of professor of nutrition has been conferred on **Dr A. E. Bender**, in respect of his post at Queen Elizabeth College.

## Appointments

**Dr Lionel E. Mawdesley-Thomas** has been appointed director of the **Huntingdon Research Centre**.

**Mr H. A. Maxwell**, **Professor D. H. Valentine**, **Mr Hew Watt**, **Professor A. D. Bradshaw** and **Professor C. D. Pigott** have been appointed members of the **Nature Conservancy**, in succession to **Sir Henry Beresford-Pierse**, **Mr R. B. Verney**, **Professor R. O. Buchanan**, **Professor A. R. Clapham** and **Sir Charles G. Connell**. **Mr A. E. Smith** has succeeded **Mr R. B. Verney** as chairman of the Conservancy's Committee for England and **Professor K. M. Clayton** and **Mr Stanley Cramp** have joined the Committee, from which **Professor J. T. Coppock**, **Dr S. M. Walters** and **Mr J. R. Herbert** have retired.

## Miscellaneous

Entries are invited by the **Plastics and Polymer Group of the Society of Chemical Industry** for the annual prize of £50, awarded for the best paper received on any aspect of pure or applied polymer science. Entrants who are not British subjects must be overseas members of the Society, and the entries should be between 3,000 and 5,000 words and in English. Further information is available from the Plastics and Polymer Group, Society of Chemical Industry, 14 Belgrave Square, London SW1X 8PS.

A travelling scholarship of £300 is offered by the **Institute of Electrical and Electronics Engineers** for a visit or visits to foreign electrical or electronic research or manufacturing establishments by a postgraduate student. Candidates must submit a programme for their visits by December 31, and must be students or

graduate members of the Institute of Electrical and Electronics Engineers, the Institution of Electrical Engineers or the Institution of Electronic and Radio Engineers. Further information is available from **Dr A. J. Ellison**, Queen Mary College, Mile End Road, London E1 4NS.

Applications are invited by the **International Agency for Research on Cancer**, Lyon, for training fellowships in 1972-73. The fellowships are awarded for one year and are tenable at a suitable institution abroad. Applicants should be junior scientists who intend to pursue a career in cancer research and who wish to be trained in such research, and the agency is particularly interested in receiving applications for training in epidemiology, biostatistics and all aspects of environmental biology. Further information can be obtained from the chief of the Research Training and Liaison Unit, International Agency for Research on Cancer, 16 Avenue Marechal-Foch, 69 Lyon (6e), France. Applications must reach the agency not later than February 29, 1972.

The 1971 awards of **State Prizes of the USSR** for science and technology include: **L. D. Fadeev**, for his work on the non-relativistic quantum theory of the scattering of charged particles; **E. P. Aksenov** and **G. N. Duboshin**, for their work on the perturbed motion of natural and artificial satellites and of asteroids, and on the formulation of the Earth's gravitational field; **Academician L. A. Artsimovich** and his team for their theoretical and practical work on high-temperature thermonuclear plasma; **Corresponding-Academician D. I. Blokhintsev** and his team for work on the new fast-neutron impulse nuclear reactor; **G. I. Goretskii**, for geological work connected with major hydro-electric projects; **S. A. Popov**, **T. M. Mukhamedov** and their team for investigating the engineering problems involved in the Bakhsh irrigation scheme in Tadzhikistan; **Yu. A. Eliseev** and **A. V. Karagin** for innovations in building technology; **G. E. Ovsepyan** and his team for development of the "Nairi" computer; **T. M. Zakharenko**, **A. K. Miloserdnyi**, **I. K. Pokhodnya**, **N. V. Panyushkin**, **V. Kh. Kas'yan**, **I. I. Kershanskii**, **I. P. Brovin**, **P. D. Borodin**, **A. I. Antonov**, **F. S. Borisov**, **I. A. Figurovskii**, **G. B. Karapetyan**, **V. L. Arkhangel'skii**, **I. M. Petukhov**, **S. G. Avershin** and their teams for specific innovations in industry; **Academician G. A. Razubaev** and **N. S. Byazankin** for work on the synthesis of metallo-organic compounds; **K. K. Andreev** and **A. F. Belyaev** for theoretical work on the combustion of explosives; **G. P. Badalov**, **Ya. A. Timoshenko**, **A. T. Vorontsov**, **E. I. Zakharova** and their teams for the development of agricultural machinery; **Academician N. D. Ierusalimskii**, **Corresponding-Academician G. K. Skryabin** and their teams for work on the production of nutrient protein from petroleum wastes; **F. V. Krogius** and his

team for their detailed study of the ecology of Lake Dalnoe (Kamchatka); **E. V. Schmidt** and his team for work on the major vessels of the brain; **Academician B. V. Petrovskii**, **Dr V. I. Shumakov** and their team for the development of kidney transplants; **A. P. Andriyashev**, **E. I. Tolstikov** and their team for their "Atlas of Antarctica"; **Corresponding-Academician R. I. Avanesov** and his team for their work on the linguistic geography of Byelorussia; **A. P. Dul'zon** and his team for their work on the Ket language of Western Siberia and the ethnology of the area; **T. Kh. Margulova** for her book *Atomic Power Stations*, and **A. P. Kreshkov** for his book *Principles of Analytical Chemistry*.

## International Meetings

January 3-5, 1972, **The Teaching of Numerical Analysis**, Brighton (The Secretary and Registrar, Maitland House, Warrior Square, Southend-on-Sea, Essex SS1 2JY).

January 4, 1972, **Plant Constituents of Pharmacological Interest**, London (Dr J. B. Harborne, Botany Department, The University, Reading RG1 5AQ).

January 4-5, 1972, **Incineration of Refuse and Sludge**, Southampton (Mr M. A. McSweeney, Symposium on Incineration of Refuse and Sludge, Department of Civil Engineering, University of Southampton, Southampton SO9 5NH).

January 4-6, 1972, **Solid State Physics**, Manchester (Meetings Officer, Institute of Physics, 47 Belgrave Square, London SW1).

January 5, 1972, **Vibration Isolation**, London (Society of Environmental Engineers, 68a Wigmore Street, London W1H 9DL).

January 5-7, 1972, **Design of Laboratories for Work with Toxic and Biological Materials**, Leeds (Director of Special Courses, Department of Adult Education and Extramural Studies, The University, Leeds LS2 9JT).

January 7, 1972, **Sampling of Mineralogical and Geological Materials**, London (The Editor, Institution of Mining and Metallurgy, 44 Portland Place, London W1N 4BR).

January 7-8, 1972, **Blood Flow Measurement**, London (Dr V. C. Roberts, Department of Biomedical Engineering, King's College Hospital Medical School, Denmark Hill, London SE5).

January 10-11, 1972, **The Molecular Basis of Biological Transport**, Miami (Biochemistry-PCRI Winter Symposia, PO Box 906, Biscayne Annex, Miami, Florida 33152, USA).

January 10-12, 1972, **Surface Physics of Semiconductors**, Southampton (Meetings Officer, Institute of Physics, 47 Belgrave Square, London SW1).



# Reports and Publications

not included in the Monthly Books Supplement

## Great Britain and Ireland

- The World Land Use Survey. Occasional Papers, No. 10: Contributions to Land Use Survey Methods. Edited by Hans Boesch. Pp. 63. (Berkhamstead, Herts: Geographical Publications Limited, 1971.) £2.50. [48]
- Shoe and Allied Trades Research Association. Annual Report 1970. Pp. 108. (Kettering: Shoe and Allied Trades Research Association, 1971.) [48]
- About Patents: Patents as a Source of Technical Information. Pp. 21. (Orpington, Kent: Sale Branch, The Patent Office, 1971.) *gratis*. [48]
- Science Research Council. Support of Enzyme Chemistry and Technology, 1969-71. Pp. 12+xii. (London: Science Research Council, 1971.) [48]
- Natural Environment Research Council. Culture Collection of Algae and Protozoa—List of Strains. Pp. 73. (Cambridge: Culture Centre of Algae and Protozoa, 1971.) [58]
- Building Research Station Digest No. 132: Wilful Damage on Housing Estates. Pp. 4. (London: HMSO, 1971.) 5p. [98]
- Political and Economic Planning. Broadsheet No. 529. ... and Now the Future: a PEP Survey of Future Studies. By Charles de Hoghton, William Page and Guy Streatfield. Pp. iii+171. (London: PEP, 1971.) £2. [98]
- Department of Trade and Industry. Safety in Mines Research 1970: Forty-Ninth Annual Report of the Safety in Mines Research Establishment. Pp. 90+4 plates. (London: HMSO, 1971.) 60p net. [108]
- Bulletin of the British Museum (Natural History). Botany. Vol. 4, No. 7: A Survey of the Tropical Genera *Oplonia* and *Psilanthele* (Acanthaceae). By W. T. Stearn. Pp. 259-323+plates 38-47. £2.60. Entomology. Vol. 26, No. 3: A Revision of the Genus *Canaea* Walker (Lepidoptera, Thyrididae). By P. E. S. Whalley. Pp. 159-179+12 plates. £1.40. (London: British Museum (Natural History), 1971.) [108]
- Association Internationale d'Océanographie Physique. Publication Scientifique No. 29: Bibliography on Mean Sea Level 1959-1969. Bibliography on Tides 1955-1969. Pp. 40. (Birkenhead: Permanent Service for Mean Sea Level, Institute of Coastal Oceanography and Tides, 1971.) £1.50. [108]
- Department of Health and Social Security Annual Report 1970. (Cmd. 4714.) Pp. xxiv+383. (London: HMSO, 1971.) £2.80 net. [108]
- Smith Kline and French Foundation. Eighth Annual Report 1970. Pp. 8. (Welwyn Garden City, Herts: Smith Kline and French Foundation, 1971.) [118]
- Metrics: a Guide to Metrication Information and Sources. Pp. 27. (London: London Borough of Lewisham, Technical Reference Library, 1971.) *gratis*. [128]
- Natural Environment Research Council—Institute of Geological Sciences ICSU/SCOR Working Party 31 Symposium, Cambridge 1970—The Geology of the East Atlantic Continental Margin. 1: General and Economic Papers. (Report No. 70/13.) Pp. v+175. 90p net. 2: Europe. (Report No. 70/24.) Pp. iv+170. 90p net. 3: Europe (continued). (Report No. 70/15.) Pp. v+106. 60p net. 4: Africa. (Report No. 70/16.) Pp. iv+209. £1 net. (London: HMSO, 1971.) [128]
- The Zoological Record. Vol. 105, Section 18, 1968: Aves. Compiled by the Staff of the Zoological Society of London. Russian literature compiled by Professor A. I. Ivanov. Pp. vi+262. £3. Vol. 106, Section 7, 1969: Brachiopoda. Compiled by the Staff of the Zoological Society of London. Pp. v+25. £3.50. (London: Zoological Society of London, 1971.) [138]

## Other Countries

- Australia: Commonwealth Scientific and Industrial Research Organization. Research Report of the Division of Nutritional Biochemistry for the year 1970. Pp. 70. (Adelaide: CSIRO, 1971.) [287]

- Australia: Commonwealth Scientific and Industrial Research Organization. Annual Report of the Division of Mechanical Engineering, 1969-1970. Pp. 67. (Melbourne: CSIRO, 1971.) [287]
- Smithsonian Contributions to Zoology. No. 72: Revision of the Fish Genus *Esenius* (Blenniidae, Blenniinae, Salariae). By Victor G. Springer. Pp. 74. \$1.25. No. 73: Synopsis of the Tribe Salariae, with Description of Five New Genera and Three New Species (Pisces: Blenniidae). By William F. Smith-Vaniz and Victor G. Springer. Pp. 72. \$1.25. (Washington, DC: Smithsonian Institution Press, 1971.) For sale by US Government Printing Office. [297]
- Smithsonian Contributions to Astrophysics. No. 13: The Variable Stars of the Large Magellanic Cloud. By Cecilia H. Payne-Gaposchkin. Pp. 41. (Washington, DC: Smithsonian Institution Press, 1971.) For sale by US Government Printing Office. [297]
- Bulletin of the American Museum of Natural History. Vol. 144, Article 4: Beginning of the Age of Mammals in Asia—The Late Paleocene Gashato Fauna, Mongolia. By Frederick S. Szalay and Malcolm C. McKenna. Pp. 269-318. (New York: American Museum of Natural History, 1971.) \$2. [297]
- Bulletin of the Florida State Museum, Biological Sciences. Vol. 15, No. 4: Catalogue of Fossil Birds—Part 4 (Columbiformes through Piciformes). By Pierce Brodkorb. Pp. 163-266. \$2.50. Vol. 15, No. 5: Some Mexican Land Snails of the Genera *Coelostemma* and *Metastoma* (Urocoptidae). By Fred G. Thompson. Pp. 267-302. Vol. 16, No. 1: Fishes of the Tortuguero Area, Caribbean Costa Rica. By Carter R. Gilbert and Donald P. Kelso. Pp. 1-54. \$0.85. (Gainesville: Florida State Museum, Museum Road, University of Florida, 1971.) [297]
- Royal Society of New Zealand. Bulletin No. 8: Cook Bicentenary Expedition in the South-West Pacific. Compiled by Ronald Fraser. Pp. 141. (Wellington: Royal Society of New Zealand, 1971.) [307]
- Journal of the Royal Society of New Zealand. Vol. 1, No. 1, May 1971. Pp. 1-86. (Wellington: Royal Society of New Zealand, 1971.) [307]
- US Department of the Interior: Geological Survey. Professional Paper 657-B: Geology of the Knoxville and Delaware Quadrangles, Johnson and Logan Counties and Vicinity, Arkansas. By E. A. Merewether. Pp. iii+18+4 plates. Professional Paper 562-F: Statistical Properties of Dune Profiles. By Carl F. Nordin, Jr. Pp. v+41. \$0.50. Professional Paper 704: Ostracode Biostratigraphy of the Yorktown Formation (Upper Miocene and Lower Pliocene) of Virginia and North Carolina. By Joseph E. Hazel. Pp. iii+13. \$0.30. Water-Supply Paper 1900: Bibliography of Tritium Studies Related to Hydrology Through 1966. By Edward C. Rhodamel, Veronica B. Kron and V. M. Dougherty. Pp. iii+174. \$0.75. Water-Supply Paper 1999-B: Availability of Streamflow for Recharge of the Basal Aquifer in the Pearl Harbor Area, Hawaii. By G. T. Hirashima. Pp. iv+26. \$0.25. (Washington, DC: Government Printing Office, 1971.) [307]
- Canada: Department of Energy, Mines and Resources. Bulletin 179: Geochemical Evolutionary Trends of Continental Plates—a Preliminary Study of the Canadian Shield. By K. E. Eade and W. F. Fahrig. Pp. 51. \$1.50. Paper 70-30: Tertiary and Cretaceous Biostratigraphic Divisions in the Reindeer D-27 Borehole, Mackenzie River Delta. By T. P. Chamney. Pp. v+43. \$3.50. Paper 70-50: Rocks and Minerals for the Collector. Ottawa to North Bay, Ontario, Hull to Waltham, Quebec. By Ann P. Sabina. Pp. viii+130. \$2. Paper 71-17: The Calibration of Gamma-Ray Spectrometers for Ground and Airborne Use. By R. L. Grasty and A. G. Darnley. Pp. iii+23. \$1.50. Paper 71-20: A Computer Program for Finding a General Distribution Function of Several Sets of Random Data, (UNIVAC 1108—Calcomp 663

- Fortran V.) By J. Soukup. Pp. v+72. \$1.50. (Ottawa: Information Canada, 1971.) [28]
- Bulletin of the American Museum of Natural History. Vol. 144, Article 6: Studies on Amphibiaenians (Amphibiaenina, Reptilia). 4. Review of the Amphibiaenid Genus *Leptosternon*. By Carl Gans. Pp. 379-464. (New York: American Museum of Natural History, 1971.) \$3.25. [28]
- US Department of the Interior: Geological Survey. Techniques of Water-Resources Investigation of the United States Geological Survey. Chapter B1: Aquifer-Test Design, Observation and Data Analysis. By Robert W. Stallman. Pp. vi+26. \$0.40. Professional Paper 733: The San Fernando, California, Earthquake of February 9, 1971—a Preliminary Report published jointly by the US Geological Survey and the National Oceanic and Atmospheric Administration. Pp. vi+254. \$2.25. (Washington, DC: Government Printing Office, 1971.) [28]
- Smithsonian Contributions to Zoology. No. 50: An Illustrated Catalogue of the Neotropical Arctiinae Types in the United States National Museum (Lepidoptera: Arctiidae), Part 1. By A. Watson. Pp. iii+361. \$4. No. 65: Units of Behavior and Complex Sequences in the Predatory Behavior of *Argiope argentea* (Fabricius) (Araneae: Araneidae). By Michael H. Robinson and J. Olazarrri. Pp. iii+36. \$0.45. No. 74: A Revision of the Nearctic Species of the Genus *Psammotettix* (Homoptera: Cicadellidae). By James F. Greene. Pp. 40. \$0.50. (Washington, DC: Smithsonian Institution Press, 1971.) [28]
- The Asbestos Research Council. Achievements 1957-71. Pp. 13. (London: Asbestos Research Council, 1971.) [38]
- Mitteilungen aus der Biologischen Bundesanstalt für Land- und Forstwirtschaft, Berlin-Dahlem, Heft 141: Pestizide in der Umwelt: Eine Bibliographie über Nebenwirkungen, Rückstände und Schutzmassnahmen. Von Dr. D. Blumenbach. Pp. 146. (Berlin-Dahlem: Biologische Bundesanstalt für Land- und Forstwirtschaft, 1971.) [58]
- An Individual Approach to Pollution. By Marilyn Karr and George Antos. Pp. 21. (Evanston, Ill.: Northwestern Students for a Better Environment, Northwestern University, 1971.) [58]
- Smithsonian Contributions to Zoology. No. 96: The Distribution of Calanoid Copepods off the South-eastern United States between Cape Hatteras and Southern Florida. By Thomas E. Bowman. Pp. 58. (Washington, DC: Smithsonian Institution Press, 1971.) For sale by US Government Printing Office. [58]
- European Brewery Convention. Report of the Barley Committee—Trials 1968. Pp. xxxvi+229. (Rotterdam: European Brewery Convention, 1970.) [68]
- US Department of the Interior: Fish and Wildlife Service. Fish Distribution Report No. 3: Propagation and Distribution of Food Fishes for the Fiscal Years 1967 and 1968. Prepared by the Division of Fish Hatcheries, Bureau of Sport Fisheries and Wildlife. Pp. ii+63. (Washington, DC: Government Printing Office, 1971.) [68]
- US Department of the Interior: Geological Survey. Bulletin 1332-B: Clinoptilolite of Possible Economic Value in Sedimentary Deposits of the Conterminous United States. By Richard A. Sheppard. Pp. iii+15. (Washington, DC: Government Printing Office, 1971.) \$0.20 [68]
- Smithsonian Contributions to Zoology. No. 94: Type Specimens in the Hans Eggers Collection of Scolytid Beetles (Coleoptera). By William H. Anderson and Donald M. Anderson. Pp. 38. (Washington, DC: Smithsonian Institution Press, 1971.) For sale by US Government Printing Office. [68]
- Encyclopaedia Cinematographica—Index 1971. Edited by G. Wolf. Pp. 137. (Göttingen: Institut für den Wissenschaftlichen Film, 1971.) [68]
- US Geological Survey. Professional Paper 632: Mineral Resources of Glacier Bay National Monument, Alaska. By E. M. MacKevett, Jr., David A. Brew, C. C. Hawley, Lyman C. Huff, and James G. Smith. Pp. vi+90+12 plates. (Washington, DC: Government Printing Office, 1971.) [98]

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